EXPERIENCE AND REFLECTION

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by E. A. Singer, Jr.

Edited by
C. WEST CHURCHMAN

Contents

PART III

Introduction

Experience and Reflection is a work which occupied most of the later years of Singer's life In effect the work represents the general schema of his philosophy, in which particular contributions dating from 1900 assume their proper relationship to one another

The manuscript was originally intended to include the concepts of "mind," "group" and "value," all of which have been treated in other works of Singer At the time of his death Singer was still intensely at work on the concept of "life," so that the original plan was not brought to its intended conclusion

The manuscript was also originally intended to include two chapters by myself, one on statistical concepts and the other on indeterminism in modern physics I have not gone ahead with this plan for several reasons First of all, the relation of Singer's work to modern statistics and physics can be stated rather briefly and, I think, adequately in this Introduction Second, it seems far better not to interrupt the author's sequence of ideas with what is really a footnote or parenthetical remark Third, the injection of my style into the middle of Singer's would certainly be incongruous

In order to accomplish the two tasks which I agreed to undertake—the relevance of statistics and indeterminacy— I think the best course of action would be to give a summary of Singer's work as I conceive it, and then to make whatever remarks seem to me to be appropriate on these two topics within the context of such a summary.

Experience and Reflection is concerned with the problem
of the correlatives, "question" and "answer." The first
chapters of the work consist of an examination of the history
of modern philosophy, in order to gain whatever assistance
one can from the reflections of the past on this critical
problem of all presents. One outcome of this examination is
Singer's conclusion that there are no questions which are
fully understandable and have completely verified solutions.
This in itself is not a very unusual conclusion for virtually
every modern philosopher who has carefully studied his
history books would assert the same thing.

But what Singer makes of this conclusion is something
else again. For one thing, the meaning he gives to the con-
clusion is certainly not a commonly accepted idea of the
present day. The reason why answerable questions do not
exist now is that in order to pose a question one must
always pose another question which cannot be directly
answered. And in order to answer this second question, one
must pose still a third, which cannot be directly answered.
This is a mere paraphrase of Singer's "There can be no
mind without another mind that reflects upon it." There
can be no question without another question that reflects
upon it.

For a philosophy of science which accepts this position,
one of two avenues of thought seem to be possible. One can
admit that all the issues of science are open to question, but
argue that some issues are simpler and clearer than others,
and that one's conclusions concerning these issues can safely
be taken as the relative basis of scientific verification. This
position is tantamount to saying that science reflects least
on that which bothers it least.

Or, one can let reflection lead where it will, and see what
becomes of the world of scientific inquiry when reflection is

given full sway This is what Singer has done, and the first chapters of this book are an historical defense of the extremely difficult choice he has made

The first thing that may catch the eye of one who takes Singer's choice seriously—and a startling thing it is—is that the science of today or any day never makes assertions in the indicative mood—but rather in the imperative. Science, being human, must act, but being fallible cannot assume that its actions are solutions It therefore acts as though a response to a question were valid Put otherwise, it sets down the command *"let* the distance measured be taken as lying in the interval $(m \pm p)$ "

This means that the logic of contemporary science is the logic of imperatives

Is there any need for a logic of assertions in the indicative mood? The study of this question is the task of *Experience and Reflection* It is the story of the manner in which the "let be taken" of the imperative becomes the "is" of the question's answer Neither question nor answer exist now, yet each exists somehow "in the limit" How is this possible? The task of this book is to set forth the kind of world of inquiry that must exist if the real answers to questions are to be conceived as the limits of a series of imperatives that science poses

Central to the process of science is the notion of control The scientist needs to know that his imperatives are "reasonable"—i e , that their acceptance provides the best chance of enabling the process to continue Of course again the scientist cannot know finally the correct control procedure, but he can make an estimate of optimal decision rules

Singer has developed an example of one such decision rule, based primarily on statistical concepts (actually, a kind

of "analysis of variance") If the scientific process can be defined in terms of a series of readings on the "same" objects, then by appropriately subdividing the series, one can test for statistical consistency This is very similar to the control procedure which W A Shewhart adopted in his study of the meaning of process control in industrial production Indeed, there seems to be essentially no difference conceptually between the control of a sequence of production items and the control of scientific readings

It should be emphasized that the theory of control of a process is still very much in its formative stages It is actually a very important part of the general theory of decisions, and even a casual acquaintance with the current status of this theory will convince one that there is no common body of accepted doctrine Hence, neither Singer's decision rules nor those of Shewhart should be considered as the final word In both instances, the decision rules make assumptions about the nature of the process that may make them less than optimal in case the assumptions fail

In Singer's case at least, this can hardly be considered a serious defect *Experience and Reflection* is concerned with the manner in which the process of science can be viewed within the general framework of decision processes, it is not primarily a book on optimal decision rules Singer's "test of responsiveness" is therefore primarily illustrative of a decision rule, it is a "feasibility" proof for his philosophy

Furthermore, I do not believe it was Singer's intention to assert that scientists "ought" to accept a decision rule similar to the one which he proposes It is well known that none of the sequences of readings for any of the physical constants such as the velocity of light satisfies even fairly loose criteria of statistical consistency But a philosophy that conceives of science as the vast and endless struggle to make its impera-

tives more closely approximate the real, must also conceive of at least one decision rule by means of which such a concept is possible

It is for this reason that I make very little comment on the test of responsiveness which Singer proposes I see nothing wrong with it as an example of a control rule, but in the face of a rapidly expanding literature on statistical control concepts, I would certainly hesitate to call it "best" For one thing, the repeated application of the test discussed in Chapter 11 ignores the compounding of errors

An important question concerning the Singerian philosophy is whether the "limits" of the scientific process "really" exist In Singer's hands, this question receives a totally different consideration from that given it by the nineteenth century optimists They could think of science as filling a vast store house of knowledge with facts, just as a Benthamite could think of an enlightened society as filling up the warehouse of man's life with pleasures One can talk happily of the limits of science, the addition of the next decimal place, and the like, when one is so clear as to what science knows and doesn't know But in the more realistic environment of comprehensive uncertainty, what shall we say about scientific progress, or the limits of knowledge which science can probe?

Here in a way Singer's approach to the problem is not vastly different from Kant's, and it has always seemed to Singer's students that he gave a far more reasonable interpretation of Kant than has been given by British and American empiricists It seems to be a totally erroneous reading of the first *Critique* to say that Kant wanted to impose *a priori* laws upon the facts of experience, so as to bring order out of chaos Rather, there could be no "facts" in any acceptable sense, unless the mind had imposed such order

Many who have recognized this much sense in Kant's effort, have felt that little is to be gained by the idea, because all we know is that the world of human experience is ordered by *some* space-time-causal scheme, but *"which one?"* is the crucial question which science raises, and the purported argument of Hume, that fact can never irrevocably decide the issue, still supposedly stands

Yet Singer has actually strengthened the Kantian position, while denying the immutability of the *a priori* For as long as science's sentences are in the imperative mood, there are no facts, and the entire enterprise hangs in balance When science reaches the point where its commands no longer meet the requirements of its own criteria of control—whatever they may be—the entire enterprise is in a precarious balance indeed It feels itself lucky to find *one* "solution" which will permit it to go on The enterprise of science can be conceived as a grand attempt to find sufficient factual support for one theory It can just as well be considered the lofty struggle to find one theory that will justify our belief in a single fact Science struggles to find one *a priori* that justifies its faith in the existence of one fact

Since the enterprise of science is taken to be so closely knit, the philosopher must proceed to examine how this is possible, especially in the light of historical developments that have cast doubts on the meaningfulness of the attempt

The development of indeterminacy in modern physics has amounted to taking uncertainty as a basic concept of physical science At first sight it seems that a proponent of any of the stated forms of an "indeterminacy principle" must fail to accept certain aspects of Singer's analysis of science For example, if the simultaneous position-and-momentum of a particle is meaningful, then it seems as though with increase of the number of observations the series of adjusted

position-readings or the series of adjusted momentum-readings must become non-responsive in Singer's sense. That is, it appears as though from a Singerian point of view one should decide that control no longer exists, although no such decision is made by the modern physicist.

This again was one of the points which Singer had hoped I would discuss within the body of his text. But quite apart from a strong reluctance to tackle the very complicated concept of indeterminacy in modern physics, I cannot see that it is really relevant at the present time to Singer's work. Perhaps the relevance will appear when both the theory of control in science and the theory of indeterminacy in physics have been given a more definitive form.

At the present time it seems reasonable enough to say that the question about the simultaneous position and momentum of the particle is not meaningful and is at best a *façon de parler*. There is a set of physical constants in quantum mechanics as well as classical mechanics; relative to this set there is no "indeterminacy." Furthermore, all assertions of quantum mechanics can be expressed in a language that makes no reference to "indeterminate" quantities. If these statements just made are valid, then the issue of indeterminacy—so-called—does seem to be irrelevant with respect to Singer's program. (For further remarks, see the Editorial Note appended to Chapter 17.)

Of course one may be unhappy with indeterminacy on other grounds. One may think that the common sense view that particles really have a position and velocity is ultimately unquestionable, so that quantum mechanics has given up the idea of answering all "sensible" questions in the limit. I am not sure what Singer's personal viewpoint would have been on this, but I think that Singer himself was far more disturbed about the discontinuity of path which is accepted

in some theories of the electron, because he could not see how then a principle of identity over time could be established

In any case, I do think that as both theories are understood today—quantum theory and control theory—there is no essential conflict between these theories and Singer's theory of knowledge

For Singer the knottiest problem to be faced was the apparent need to partition science into at least two parts, because of the supposedly inherent impossibility of adjusting the observations of living bodies to any physical image of nature It is clear on the one hand that in order to establish control in the physical sciences one has often to resort to the human observer and his properties The physicist needs to "sweep in" the observer But it is also clear that the attempts to define life so that the properties of living things follow from the laws of physics is doomed to failure This problem, which Singer elsewhere calls "mechanist-vitalist," is considered in general in other writings In this book Singer undertakes to do what these other writings suggest can be done the construction of biological concepts which are at once "independent" of physical imagery and yet never conflict with physical imagery

Below I've cited certain Singerian writings which carry his program beyond biology

In editing this work for publication, I have left the material just as the author wrote it, except in a few minor instances where his final corrections were not clear, and except for the final two chapters Singer had carefully gone over the entire manuscript through Chapter 21 years before his death The final two chapters were not left in definitive form, some of the material being in older drafts, some on tape I've done what could be done in terms of

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available material and my own understanding to fill in the
gaps and make a coherent manuscript. Except for Chapter
23, places in which I have filled in parts or all of sentences
have been so designated by brackets.

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Bolinas, California   C. WEST CHURCHMAN

Selected Additional References to Topics Beyond Biology

Articles:
"On Sensibility," *The Journal of Philosophy*, 1917, Vol XIV
"On Spontaneity," *The Journal of Philosophy*, 1925, Vol XXII
"On the Conscious Mind," *The Journal of Philosophy*, 1929, Vol XXVI
"Mechanism, Vitalism, Naturalism," *Philosophy of Science*, 1946, Vol
    XIII.

Books:
Modern Thinkers and Present Problems, Henry Holt & Co , 1923
Mind as Behavior, R C Adams & Co , 1924
On the Contented Life, Henry Holt & Co , 1936
In Search of a Way of Life, New York. 1947

Part I

1. Foreword

MAN'S EVERY ACT IS AN ACT OF FAITH, GENERALLY, AN
unconscious faith, seldom, an examined faith, never, a faith
that by taking thought could have been replaced by assured
certainties All this appears whenever we stop to "rational-
ize" our conduct Whatever that conduct may have been, it
can be made to appear reasonable only if some propositions
are taken to be true, others false Or, perhaps the more
experienced and less exacting mind would feel the reason-
ableness of its actions to have been as well established as
might be, if it could show the premises with which its acts
were consistent to be more likely true than false, those with
whose truth they were inconsistent, more likely false than
true Either way of putting it points to the one moral,
namely, that, apart from incalculable chance, there is no-
thing but one's judgment in the matter of premises to deter-
mine all that one's life can come to know of fruition or
frustration Nothing, then, can be more important to any
man than to do all he can to assure the soundness of his
reasons for the faith that is in him, i e to test the weight of
evidence supporting the working hypotheses on which he is
willing to act But to have considered the evidence for the
hypotheses on which he has acted or is prepared to act,
could be practically useful only to one who had in one way
or another come by a sound idea of the evidence on which
one should act, if one's actions are not to end in disappoint-
ment The result at which a man's thought on this matter of
evidence may have arrived, will constitute his *theory of*

evidence, and the task of constructing the soundest possible theory of evidence is the undertaking of *philosophy*

At least, this is the sense, and the only sense, in which the term *philosophy* will be used throughout the present study. That philosophy has not always meant just this, and does not now mean this to all "philosophers", is of course true But then, there can never be any objection to restricting one's use of a term that in the course of time has taken on bewilderingly many connotations, to just one of its historic meanings, provided, of course, one is careful to specify beforehand just what his choice in the matter is to be And this choice will not be thought arbitrary, whether or not approved as wise, if the motive that determined it be pointed out at the beginning Now, the motive that decided the present study to restrict its use of so old a term as *philosophy* to just one of its manifold historic meanings, is itself an historian's motive Briefly, it takes the theory of evidence to be not only *a* problem with which the "philosophers" of the past came to be more and more concerned as reflection advanced, but *the* problem that in modern times has come to be the distinguishing study of philosophy And this understanding of what philosophy has come to mean would seem to be shared by most contemporary historians of philosophy in its development Likenesss and difference of answers given to the question "What constitutes evidence?" serves them as *fundamentum divisionis* on which to classify modern schools On this basis, they would include all schools that had appeared before the nineteenth century opened under one or the other of three widely accepted names Rationalism, Empiricism, Criticism In this classification, those philosophers who show a like conception of evidence are placed in the same school however different their final world-views, those whose conceptions of evidence differ are

assigned to different schools, however like their world-views.

Here, one may wonder how it comes that thinkers who accept the same *modus inveniendi*—the same way of finding out things—should nevertheless have come to find themselves in different kinds of world, while those who follow different ways of finding, end by finding their worlds to be much of a kind. This could not be, of course, if two conditions were fulfilled, if, namely, it were possible for the human mind consistently and persistently to restrict its beliefs to those which its own understanding of evidence fully supported, and if, again, it were possible for the mind of man to frame a theory adequate to decide all questions of evidence that might be meaningfully put to it.

But neither the gift of perfect consistency, nor that of ultimate penetration has been accorded the finite mind, had both been given us, there might be no reason left for calling our minds "finite" And this is why the theory of evidence, as thought out by finite minds, has a history. As the minds that have made this history may be counted among the best humanity has produced or is likely to produce it seems only wisdom on the part of one who would think out his own theory of evidence to profit as fully as may be by the experience of the thoughtful past, follow the history of its hopes and disappointments, its disillusionments and consequent enlightenments. At all events, the present study of evidence *has gone on that assumption, and begins by recalling, for its* own later guidance, what it takes to be the most important, though by no means the only important moments of so much of this history *as falls within* "modern times"

For the use to which this historical introduction is to be put in the sequel, "modern times" may be allowed to begin with a moment in history—a rather marked moment as it

happens—when the more critical question "How do we
know?" is given precedence over the more practical question
"What do we know?" It was, naturally enough, this more
practical question that had preoccupied the earliest and
predominantly interested the later years of the first two
millenia of "philosophy's" history, from the Miletians of
the sixth century B C to the moment in the seventeenth cen-
tury of our era, when Rationalism appears But, of course,
thoughtful minds could not have gone on considering this
practically important question for 2,000 years, without
successive thinkers having turned more and more critical
attention to the trustworthiness of the evidence on which
their opponents had based their conclusions, comparing this
evidence unfavorably with the soundness of the tests of
truth on which they themselves depended So that it would
be a mistake to suppose "modern times" to make an abrupt
break with all that had gone before, or to take too seriously
the "cleanness" of the slate on which the first of the
modern schools conceived itself to be setting down its brand-
new beginnings If there had been any such discontinuity
between the "old" and the "new" as the "new" was in-
clined to pretend, we should lose more than we could well
afford to forego in beginning, as we shall, the history here
to be reviewed with the mid-seventeenth century Perhaps
it would be safe to say that what we accept as the mark of
"modernity" lies rather in the arrangement of a modern
philosophic study, the order in which its problems are dis-
cussed, than in the problems themselves or their several
solutions It is at any rate to be noted that most historians of
philosophy select as the first contribution to its modern
period, one who was at pains to prepare the way for his final
systematic account of what he held to be true, by a careful
study and determination of the way one must take to find

out what was true Thus, we find this first modern, René Descartes (1596–1650) preceding his most comprehensive work, *Principles of Philosophy* (1644) with two examinations of the test of truth, *Discourse on Method* and *Meditations on First Principles* (both, 1641), and introducing the *Principles* itself with a like consideration of method There is good reason, then, for a study exclusively concerned with an examination of evidence, interested in results only in so far as they reveal the method by which they were arrived at, to be content to let its introductory review of historic schools begin with the seventeenth century Rationalism, founded by Descartes

And if our history of method may begin thus late, so, for the principal use to which it is to be put in the sequel, it may be allowed to end relatively early For, the sole purpose of this recalling of the past is to let the past furnish its own evidence as to what in the way of evidence-theory has already been weighed in the balance and found wanting But if we are to accomplish even this much effectively, it is best not to attempt too much A preliminary review of our modern schools, if it is to display anything of the historic reasons for their successive coming into being and passing away, will do best to limit its reconstruction to a few of those earliest to develop And, in fact, the review offered in the present study attempts to recall no more of modern history than the period of some 150 years in which appear the first three theories to be formulated, namely, Rationalism (seventeenth century), Empiricism (eighteenth century) and Criticism (late eighteenth and early nineteenth century) What one would lose in stopping his reconstruction of the past short of the moment at which all three of these schools of thought had found their full expression, will show itself quite definitely in the course of that reconstruction

tself What one may have lost in following history no
further will be only temporarily lost. for, no constructive
study in any field of inquiry should consider its work
finished until it had differentiated its conclusions, not only
from *some* theories of the past, but from *all* theories offering
themselves at the moment of its completion. To meet this
demand, is likely to prove one of the most difficult problems
facing any constructive effort. and, of course, it is a task that
cannot be undertaken until the construction itself has
written its last word. As for the historical introduction
offered in Part I, it will, one thinks, have rendered all the
service a reconstruction of the modern past can profitably
undertake in following as carefully as possible the motives
that brought into successive being the three theories of
evidence that together exhaust the independent offerings
of the first century and a half of modern times, in showing
in what these theories are incompatible each with each. and
in suggesting the conditions to be fulfilled by any fourth
theory that shall be as incompatible with any of these three,
as each of them is with each other To that limited but clear-
cut end, Part I could find no more economical and effective
means than the method of reconstruction which it calls a
dialectic

Although the first suggestions of a modern "dialectic
method" are to be found in Kant (his "Antinomienlehre"),
the method did not claim for itself a place of commanding
importance until generalized in the systems of Fichte and
Hegel Since then, and particularly in our own day, it has
been taken by some to have a universality and rigor that
would make it the supreme and ultimate *modus inveniendi*,
the final finding of man's long search for an infallible way
of finding out things The place to be accorded this "Hege-
lian dialectic" (with its Post-Hegelian variants) in an

exhaustive classification of truth finding methods, is one of
the issues to be faced in our final retrospective survey of
history It is not to prejudge the issue, to explain at once that
the "dialectic" of our phrase, "Dialectic of the Schools,"
has no claim to a place in any such classification The merits
that recommend its use here are purely psychological, quite
without pretense to epistemological significance Since
Plato's time, such dialectical procedure as is here used has
been found a good way of thinking things out, whether the
thing in question is itself the problem of an ultimate criteri-
on of truth, or whether it is a special problem to be worked
out in the light of some accepted test of truth. Every one
will recognize in the dialectical ordering of this study the
way his own thought has often worked in thinking things
out for itself No need, then, to say more beforehand con
cerning a form of progressive debate that everyone will find
familiar, the moment he is presented with an example of its
use. To this example, to this use in the Dialectic of the
Schools, we now turn.

2. Inexorable Law : Rationalism

THE CYNICS TAUGHT THAT THE ONLY WAY TO AVOID defeat was to keep out of battle, so rarely are our wishes fulfilled, it is better to throttle desire than to let it be disappointed. Whatever we may think of this cynic wisdom, we at least recognize the facts from which it sets out, for life is a rich experience in obstacles—so rich, we are inclined to look upon resistance to our will as the very quality and criterion of the real

But if we ask, *What* most opposes us in the world? we receive not one answer but two. For some would say, What is unyielding is law, and others would answer, It is fact. Now *law* and *fact*, however we confuse them at times, have been carefully distinguished by those who have tried to think clearly. The two are of different flavor, as any one may find for himself by putting them to a simple test.

What emotion, pray, does it awaken in your breast to repeat to yourself "All men are mortal"? None at all, it is likely, or at most a very mild one. Yet this is the *law* of death and might be supposed full of tragic meaning. It is indeed the bearer of an ugly threat, do we but think of it. For if all men are mortal, then *this* man who is so dear to me must die, and to face this fact, if imminent, may well wring the stoutest heart. What affects us is not death, but *this* death. Bossuet has somewhere said "We all know we are to die, but none of us believe it." Is there not a difference here? It in no wise touches us that innumerable hordes of men have died and countless more will die. But someday the

news, "*He* is dying," he whom you so love, may come to strike you with the force of a blow.

In a word, law is *universal*, fact is *individual*; and those who think of the resistant as "inexorable law," and those who think of it as "hard fact" belong to different schools of thought with different traditions behind them. So that if we are to examine the opinion which holds the real to be the resistant and the resistant to be the real, it will be well to enquire separately into the "inexorability" of law, and the "hardness" of fact.

If we begin with law, a difference between two kinds of law presents itself for consideration. When I say, Self-preservation is Nature's first law, I may be supposed to mean that holding on to life is an end to which all other ends must yield. The law then is a law of purpose. But when I say, Gravitation is a fundamental law of Nature, no comparison of ends is in my mind; for gravitating is not a purposeful kind of behavior on the part of falling bodies.

Each of these two kinds of law, the one teleological, the other ateleological, has been taken at times to be inexorable. One would feel the pull of self-preservative purpose in a moment of panic, when the cry of *sauve-qui-peut* had gone out. Let one totter over the edge of a precipice and the pull of blind gravity is all one would know. The two kinds of compulsion put upon us by Nature have been thought very much alike, and certainly each has an inexorability of its own against which it is hazardous for man to pit his will and feeble strength. Yet, of the two, the ateleological aspects of Nature, to which its mechanical laws belong, are generally felt to be even less alterable than the teleological. For where we know teleological law by long experience, we find it to be of the kind that admits of exceptions—holding, as

Aristotle would say, only "for the most part." That self-preservation is the first law of Nature is none the less true because voluntary abandonment of life is a common enough occurrence. No teleological law of Nature that we know of precludes the accidental, and it is of such "rules" that it may be said, they are proved by the exception. But any appearance of an exception to a law of physics (or of any other ateleological science) at once puts that law out of court until it shall have been purged of its appearance of irregularity.

If then we would know how obdurate is law, we should take it at its worst, or best—that is, in the domain of the "exact sciences" Such was evidently the feeling of Descartes, who was inspired to his whole conception of truth and of the method by which truth is to be arrived at by the example of mathematics, "on account [as he said] of the evidence and certitude of their reasoning." "And thinking [he continues] that they were but contributing to the advancement of mechanical arts, I was astonished that foundations so strong and solid should have had no loftier superstructure reared on them On the other hand, I compared the disquisitions of the moralists to very lofty, towering and magnificent palaces with no better foundation than sand and mud   "[1]

This "loftier superstructure" which Descartes and the other Rationalists who followed him—Spinoza, Leibnitz—labored to raise on the sound foundation of mathematics is the great historical example of the opinion we have undertaken to examine first—the opinion that the criterion of all truth is the same in kind with the test of mathematical truth; that no law and no fact is known to be real unless we can be convinced of its reality by a demonstration as rigorous as that which a geometer requires for any proposition

he is to accept as true. And the Rationalists conceived that all truth could be established in just this way, even the true solution of those most baffling problems of morals about which men are so various in their opinions, and for their opinions invoke such various authorities. To construct an "ethics after the manner of a geometry" was Spinoza's ideal; an ethics whose laws should be as like as possible to the theorems of Euclid and as unlike as possible to any laws of human enactment. So then, if we are to try out the truth of this opinion, that law has a validity of no human making or possibility of changing, we cannot do better than follow the reasoning of those who believed this most thoroughly, the Seventeenth Century Rationalists.

If we do—if we turn as the Rationalists did to the science of geometry for our model of "clearness and evidence" we find there a rich array of propositions respecting the relations of points, lines and surfaces in space, which purport to be deduced from a very few statements of such relationships. These premised statements are so simple in their nature that any child could understand them and realize their truth, e.g., "All right angles are equal", "figures that can be superimposed are equal."[1] As for the method by which the complicated system of propositions constituting the doctrine of geometry may be deduced from these few simple premises—this too can be shown to depend upon a few obvious formulas; such as, All a is a, If all a is b then some b is a, If all b is a, and all c is b, then all c is a. These propositions are said to be true whatever terms (substantives) the a, b, c . . . may stand for, and the science whose task it is to construct all propositions whose truth is thus independent of the meaning of their terms is *logic*. It is assumed that by allowing the substantives which appear in the postulates of geometry to be substituted for the a, b, c . . .

of logic, the theorems making up the body of geometry may
be "deduced" or "demonstrated."

Where in such a system could the most scrupulous doubt
find a loose end to lay hold on? Not surely in the formulas of
logic by which deduction is effected, nor yet, it would seem,
in the postulates of geometry from which deduction sets out.
Small wonder that the mind moves with such singular ease
and confidence through the pages of a treatise on geometry,
and small wonder that the rigor-loving mind of the Ration-
alist should have conceived the ambition to accept as true
only such propositions, even in the baffling domain of
morals, as could be deduced from premises no less evident
than are the premises of geometry.

Nevertheless, although both the axioms of geometry and
the formulas of logic appeal to us with a very great force, yet
the rigorists of the seventeenth century realized that the
absurdity of doubting that "All right angles are equal" was
a trifle less obvious than the nonsense of doubting that "All
a is *a*." Of course a doubt to be real must be something
more than the words "I doubt", which anyone may append
to any proposition at no expense of thought. But to conceive
a real doubt is a very thoughtful undertaking, the words
must be shown to stand for an act of the imagination, there
must accompany them an effort, often very severe, to con-
struct an image of a world in which some state of affairs
differing from the one we are trying to doubt would present
itself to our understanding. Only one who has tried to frame
for himself the image of a world in which right angles are
not equal, can let amusement get the better of admiration
when he reads Descartes' appeal to God to help him to an
unbelief in geometry. "We have learned [he writes] that
God who created us is all-powerful" and "we do not yet
know whether perhaps it was His will to create us so that

we are always deceived, even in the things we know best "[5]

In the context, Descartes is seen trying to shake any confidence we may have in our judgment of anything, "even the things we know best " Of all our beliefs, the hardest to threaten are our mathematical tenets, and in this perplexity as to how we might be made to feel their insecurity, his thought casts back to a question the medieval schoolmen often pondered "God [they posited] is omnipotent An omnipotent being can do anything Cannot God, then, perform contradictions? Can He not, for example, create square circles?" But, no, they answered, it is no limitation of omnipotence to admit that it cannot perform what it is meaningless to propose

When, then, Descartes reflects that we do not know whether perhaps it was God's will to create us so that we are deceived into believing "all right angles to be equal" when in truth they are not, his meaning might be put in this way We do not yet see that to deny equality to all right angles involves a contradiction, and only that to deny which involves a contradiction is beyond the reach of doubt If this test by which we may distinguish necessary or indubitable truths from all other matters of opinion never came to clear formulation in Descartes, it did in the later thought of Leibnitz From it, *Rationalism is led to some very interesting conclusions respecting truth in general, the axioms of mathematics in particular*

For it came at last to be quite clear to the Rationalist that the only type of proposition to deny which involved a contradiction was what Leibnitz called an "identity," or what we are accustomed to class as a "definition " Only when it is granted that a square is *by definition* a four sided figure can we answer Descartes' question, "How do I know that I am not deceived each time I number the sides of a square?" [6]

of logic, the theorems making up the body of geometry may be "deduced" or "demonstrated."

Where in such a system could the most scrupulous doubt find a loose end to lay hold on? Not surely in the formulas of logic by which deduction is effected; nor yet, it would seem, in the postulates of geometry from which deduction sets out. Small wonder that the mind moves with such singular ease and confidence through the pages of a treatise on geometry; and small wonder that the rigor-loving mind of the Rationalist should have conceived the ambition to accept as true only such propositions, even in the baffling domain of morals, as could be deduced from premises no less evident than are the premises of geometry.

Nevertheless, although both the axioms of geometry and the formulas of logic appeal to us with a very great force, yet the rigorists of the seventeenth century realized that the absurdity of doubting that "All right angles are equal" was a trifle less obvious than the nonsense of doubting that "All *a* is *a*". Of course a doubt to be real must be something more than the words "I doubt", which anyone may append to any proposition at no expense of thought. But to conceive a real doubt is a very thoughtful undertaking; the words must be shown to stand for an act of the imagination; there must accompany them an effort, often very severe, to construct an image of a world in which some state of affairs differing from the one we are trying to doubt would present itself to our understanding. Only one who has tried to frame for himself the image of a world in which right angles are not equal, can let amusement get the better of admiration when he reads Descartes' appeal to God to help him to an unbelief in geometry: "We have learned [he writes] that God who created us is all-powerful" and "we do not yet know whether perhaps it was His will to create us so that

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Only then is a contradiction involved in giving a number
other than four to those sides; only then is the proposition,
"All squares are four-sided", indubitable. So Leibnitz was
quite clear that the axioms of geometry are definitions; not
indeed the axioms as Euclid laid them down, but the axioms
as they remained to be formulated by a keener analyst than
Euclid.

> The same motives [he writes] that would make
> Euclid try to prove that two sides of a triangle are
> greater than the third (although the truth of this is so
> evident to experience that, as the ancients jokingly
> said, even asses recognize it in not taking round-about
> roads to their mangers)—the motive, namely, that
> Euclid wanted geometrical truths to depend not on
> sense-images but on reason—these same motives
> would make one wish to prove that two straights can
> intersect at only one point which Euclid could have
> proved *si bonam rectae definitionem habuisset*.[5]

To sum up, the Rationalist's argument runs in this wise:
No contradiction appears in denying the hardest fact known
to us by observation, such knowledge may always be
doubted. And no less open to doubt must be any empirical
rules generalized from such observed facts. Resting their
appearances of universality on induction, these rules can
obviously be no more hard-and-fast than are the facts on
which they depend. But laws, "necessary truths" as the
school called them, are as inexorable and undeniable as the
principles of logic by which they are established. By which
alone they are established,—for it comes to that.
As independent principles, the axioms of the special sciences
will have disappeared, only logic remains as the *modus
inveniendi*—the unique method of attaining to truth.

All this would seem to have put science beyond the reach

of those misadventures with which empirical generalizations
are constantly meeting moments when the new experi-
ment of to day upsets all that previous experience had
taught us to hold to most firmly But new experience cannot
upset definitions or a science based on them, for, as Leibnitz
says, such a science "ne dépend que de nous memes, elle
n'a que faire des secours exterieurs"⁶ But we have no
sooner said this and set ourselves to consider all that it
means, than we begin to wonder what has become of that
"inexorability" of law which Rationalism set out to prove
All law hangs on definitions, and definitions "ne dependent
que de nous memes"!

What then is this definition, whose denial involves a
contradiction? Is it anything more than a convention among
those who would hold intercourse with one another? So far
from being a "necessary" proposition, is it really a proposi-
tion at all? Is it not rather a question and an answer—or,
perhaps, a motion and a vote? Must we not, in tracing the
history of any definition, come sooner or later on an episode
like that which fixes the meaning of gram, calory, watt—
the moment when such and such a member of a collective
body rises to move "That we make such and such a thing a
unit, and call it by such and such a name"? And the con-
vention votes "Aye " Is a proposal a proposition? Is its
acceptance or rejection a proposition? Can either be true or
false?

But, of course, the convention that fixed the meaning of
our oldest and most familiar terms had neither presiding
officer nor secretary Its minutes are not before us, and it is
often a delicate matter to reconstruct its decisions or divine
its original intentions Who would like to be asked the
meaning of life, or mind? The genius of a Socrates was well
enough, yes, and fully enough employed in groping after

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the vague instinctive sense that the spirit of the race had put
into such familiar terms as *the good* and *the beautiful*
Certainly, we may argue with one another over the *true*
definition of "the good," quite as though a definition were
the kind of thing that could be right or wrong, true or false,
and this no doubt is the reason why the Rationalists felt so
little misgiving (though they did feel some) in founding
truth upon definition But if we reflect upon what we mean
when we somewhat carelessly speak of "the true definition
of *life*" or the like, we find what is here called *true* to be no
definition, but a statement to the effect that such and such
is the definition of *life* Let the statements be made that a
certain entry of a motion and vote is to be found in the
minutes of the British Association this statement may
indeed be true or false So may the statement, Euclid de-
fined a triangle as a three-sided plane figure But neither of
these statements *is* a definition, it is a statement *about* a
definition, and may be classed with such other allusions to
history as, The Congress passed a law providing etc So it is
with our "true" definition of *life*, the definition offered will
be all that we mean it to be, if it truly discover what mean-
ing the race has put into this word as evidenced by its use
It will then have added an item to our knowledge of history,
and will have been at great cost to do so, for the minutes of
the historic past are lost It will have added no item to our
science of biology So it turns out that those laws which be-
cause of their "certitude and evidence" were to furnish the
"strong and solid foundations" of all truth, so far from being
themselves true are not even false So far from having that
unyielding necessity against which human wills would
break if they contended, they are nothing but the expres-
sion of what that very human will once wanted It is only
when these laws seek application, only when these defini-

tions enquire whether there exist any objects corresponding
to them, that matters are taken out of our hands and put
beyond our election

Ordinarily, we are content to let such questions of fact be
answered by observation A chiliogon *is* a thousand-sided
plane figure? Agreed But *is there* such a thing as a chilio-
gon? We can only "look and see " But, of course, if the
Rationalist could be driven to admit that there was no other
method than this of answering questions of fact, logic would
no longer be his only guide to truth and he himself would
no longer be a Rationalist Wherefore, the Rationalists were
constrained to show that *in at least one case the existence of a
thing was involved in its definition*

The famous example of an effort to demonstrate such a
case is the "ontological proof of the existence of God "
Every Rationalist turns and returns to this proof in the hope
of making it convincing, but the argument always comes
back to the following points God is by definition a most
perfect being, a being is more perfect than another if it
possesses all the attributes the other has together with some
attribute the other lacks, the only being that lacks no
attribute another might possess is one that has *all* attributes,
existence is an attribute, therefore God, a most perfect being,
must have existence or must exist This, with many
tortured variations, is the outcome of Rationalism's attempt
to escape the doubt and uncertainty of an appeal to experi-
ence, to observed fact Only those, if any such there be, who
find the ontological proof of God's existence satisfactory can
escape the conclusion that if there is anything hard and un-
yielding in the nature of the real, it is not in universal law
but in individual fact that this stubbornness must have its
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Empiricism

¹*Discourse on Method*

²Euclid, *Elements*, Post IV, and Common Notions VII

³*Principia*, I, 5

⁴*Meditations*, I (condensed)

⁵*Animadversiones in partem generalem Principiorum Cartesianorum* Gerhardt, IV, 354 Leibnitz supposes that the axiom of "two straights" is one of Euclid's See, however, Stäkl *Gesch d Parallelaxioms*

⁶Letter, probably to Princess Sophie, against Descartes, Gerhardt, IV, 298 Leibnitz is here speaking of geometry

⁷The *feeling* of a fallacy in the ontological proof is almost universal, to give accurate expression to the reason for this feeling is not so easy One of the important historical discussions of the argument is Immanuel Kant's (Kr d r V B 619ff) Here it is maintained that the error lies in the premise, Existence is an attribute "Sein ist offenbar kein reale Predikat, die ein Begriff von irgend etwas, was zu dem Begriff eine Dinges hinzukommen konne " (B 626)

3. Hard Fact: Empiricism

IN OUR SEARCH FOR WHAT IS MOST REAL IN THE WORLD,
to have passed from law, as the Rationalist conceived it, to
facts as the Empiricist perceived them, is to have softened
nothing Well do we know the hard temper of facts As one
groping his way out of a dark room bruises himself on the
angles of things, so do we on our way through life break
ourselves against its stubborn facts Nor does law, in aban-
doning its former appeal to reason and basing itself anew on
the authority of these facts, lose anything of its rigor Death,
a "practical certainty," is no more gentle than death, a
"logical necessity "

Such at least is our first impression of the Empiricist's
world If we would test the finality of this impression, we
must begin by enquiring into the meaning of those facts of
which his world is composed The task should not be a
difficult one, for nothing forces itself on our acquaintance
more persistently than do these same facts

And indeed, if we turn to those who made the most of
facts in constructing their image of reality, we discover no
suspicion on their part of any serious difficulty in setting
forth the meaning of fact Not but that the classic Empiri
cists of the Eighteenth Century found some reflection on the
matter necessary, for the facts of our mature experience are
complex things, they were gradually learned of, and came
late to our knowledge Whatever our ripe philosophy may
hold respecting these complexities, its opinion can only be
justified after reflection made on how we came by our sup-
posed knowledge of them

And I shall imagine [wrote John Locke in 1690] I have not wholly misemployed myself in the thoughts I shall have on this occasion, if in this historical plain method I can give any account of the way whereby our understandings come to attain those notions of things we have, and can set down any measures of the certainty of our knowledge, or the grounds of those persuasions which are to be found amongst men so various, different, and wholly contradictory, and yet asserted somewhere or other with such assurance and confidence . . . Our first inquiry then shall be how ideas come into the mind.[1]

But for a Locke, a Berkeley, a Hume, this "first enquiry" proved the most straightforward of tasks. And if they succeeded thus easily in explaining "how ideas come into the mind", it is principally because they had so little hesitancy in fixing on the "simple ideas" that come first into the mind. These would be the beginnings of our experience, and it is not hard to understand the importance to an empirical philosophy of this problem of beginnings.

When first we came to "the shores of light", it is not to be supposed that we were prepared at once to recognize all the beds and baths, mothers and nurses with which these shores welcomed us. These things, which now suggest themselves to our reflections as life's first fact-offerings to the new-comer, we could not then have known for what we now take them to be. For to know a bed from a bath or a mother from a nurse takes experience, and this on the occasion of our first meeting mothers, nurses and the like, was completely lacking to us.

We quite make it out then that any Empiricism would be constrained to open with some account of our first ideas.

All our knowledge of the factual world would begin with these, which as they are our first and simplest "ideas" are also the first and simplest "facts" of our experience. Locke's proposals in the premises are such as would likely appeal to anyone; they did to Berkeley and Hume whose doctrine on this point closely follows Locke's. All three recognize two sources from which these "simple ideas" must spring.

> First, [as Locke puts it] our senses conversant about particular sensible objects do convey into the mind several distinct perceptions of things according to those various ways wherein those objects do affect them. And thus we come by those ideas we have of *yellow, white, heat, cold, hard, bitter, sweet,* and all those we call 'sensible qualities'. . . .
> Secondly, the other fountain from which experience furnisheth the understanding with ideas, is the perception of the operations of our minds within us as it is employed about the ideas it has got; which operations when the soul comes to reflect on and consider do furnish the understanding with another set of ideas which could not be had from things without. And such are *perception, thinking, doubting, believing, reasoning, knowing, willing,* and all the different actings of our own minds.

With these two "fountains of experience" Locke rests content;

> "the understanding" seems to him "not to have the least glimmering of any ideas which it did not receive from one of these two. External objects furnish the mind with the *ideas of sensible qualities,* and the mind furnished the understanding with *ideas of its*

own operations These, when we have taken a full survey of them and their several modes, combinations and relations, we shall find to contain our whole stock of ideas, and that we have nothing in our minds which did not come in one of these two ways "[2]

So much for the beginnings of experience its facts are simple ideas They may well enough be called "hard facts," not because they are hard to bear, but because, whether they prove welcome or unwelcome, the "white paper" of our new minds can neither invite nor refuse them

Afterwards, from these simple ideas of sensation and simple ideas of reflection our knowledge expands until it takes in the farthest reaches of space and remotenesses of time all "that vast store which the busy and boundless fancy of man has painted on it with an almost endless variety " But if at the beginning we lie thus passive in the arms of fate, is there nothing in the way our world grows on our hands to make us feel that it grows more amenable to our will, more responsive to desire?

> "We have hitherto considered those ideas in the reception whereof the mind is wholly passive, these being all the simple ideas received from *sensation* and *reflection*, whereof the mind cannot make *one* to itself, nor have any idea which does not wholly consist of them But as the mind is wholly passive in the reception of all its simple ideas, so it exerts several acts of its own whereby out of its simple ideas as the materials and foundations of the rest the others are framed "

These "several acts of [the mind's] own" turn out to be "chiefly these three, to wit, (1) *combining*, (2) *comparing*

(3) *abstracting* Which shows man's power and its way
of operation to be much what the same in the material
and intellectual world For the materials in both being
such as he has no power over either to make or destroy,
all that man can do is either to unite them together
or set them by one another or wholly separate them "[3]

Here indeed the mind's "acts" and "powers" are spoken
of as though the complex world that grew on us with ex-
perience might be much more of our choosing, and so more
to our choice, than the simple hard facts of its first begin-
nings But the sequel of Locke's discourse does not alto-
gether justify such an inference Of the three acts of the
mind the most significant is that of "combining," whereby
"all the complex ideas are made " Yet some at least of this
power of *combining* turns out to be no more than the power
of considering as one what we may find already combined
"As simple ideas are observed to exist in several combina-
tions united together, so the mind has a power to consider
several of them united together as one idea "

To be sure, Locke hastens to add that the mind exercises
this power to consider as one several ideas

> "not only as they are united in external objects, but
> as it itself has joined them " And a little later he again
> points out that "when the mind has once got these
> simple ideas, it is not confined barely to observation
> and what offers itself from without, it can by its own
> power put together those ideas it has and make new
> complex ones which it never received so united "[4]

Now no one would willingly deny the mind some such
power as this Even a mind that has received its simple ideas
in no other "combinations united together" than such as a

vision of slums and a burden of pain may be, can yet "of its own power" put together whole fairy lands, Utopias and scenes of Paradise But does its control over simple ideas, over the ultimate facts of experience, go much further than this ability to fall adreaming?

One remembers that the bare facts of experience are beyond our shaping, one learns that its underlying laws (of concurrence and sequence) are none of our making, one is told that if the world of our experience shows any law at all, we have only the gods to thank, or our good luck, after which one's interest in the only power the mind admittedly has over the facts of experience singularly fails But, then, that a philosophy has a dreary outlook does not mean that it has a false insight There are those who neither deny the outlook nor doubt the insight of Empiricism In particular, the scientific observer recognizes in this account of the growth of knowledge something of his own experience in learning, and feeling himself thoroughly at home in this description of the way in which the business of learning goes on, he is little inclined to be critical of the philosopher's account of the way it begins Some matters for improvement have indeed occurred to him The twofold classification of simple ideas into those of sensation and those of reflection has proved irritating, as two-fold classifications always will be to the scientific spirit, whose ideal of husbandry is to make one rose grow where two grew before Perhaps the "simple idea of reflection" will turn out to be a complex of sensations It is easy to see that many sensations do enter into what is called an emotion Why suppose anything but sensations to enter therein? And this "James-Lange theory of the emotions" suggests similar treatments of the other ideas of reflection, e g , volition with its kinesthetic sensations of stress and strain May not all the "simple ideas of

reflection" come in time to be recognized as complex ideas of sensation?

But again, even if with the "sensualists" we recognize only the "simple ideas of sensation," can these truly be illustrated by such perceptions as *"white, heat, cold, bitter, sweet"*? These mental states reveal themselves to the experimental psychologist as already complex Perhaps it would be safer to risk no name for the simplest data of these experience, but, with Helmholtz, cautiously refer to elements as *qualia*, or with the still greater caution of Mach, let α, β, γ,—stand for whatever the simple may turn out to be [5]

But if these are the only reflections time has brought to bear on the classic doctrine, Empiricism cannot have fallen into any serious philosophic error Little can it matter whether Locke, Berkeley, Hume found just the right name for the beginnings of experience, the important thing is that these men gave a sound account of how, by observation of fact, knowledge grows They showed that it could have grown in no other way, from whatever beginnings it may have started They concluded that experience must have had its beginning in a reception of facts, these, at all events, may safely be called *simple* facts If *white, heat, bitter* are proved to be no such simple facts, simpler facts must have been adduced to prove this Hold, then, these simpler *qualia* for simple, so long as there is no evidence against their simplicity Should any such evidence come to light, replace these *qualia* by whatever, to prove their complexity, must have established its own simplicity And so on It would seem that Empiricism could never fail of words in which to express its meaning As for a last word on the facts of the case, Empiricism can pretend to it the less, the more Empiricism intends to remain empirical

To the experimental scientist then, Empiricism remains very much his own philosophy It pictures infancy's first uncircumspect steps in learning as one in kind and tendency with mature thoughts own planful searchings And as this continuity with a profitable past seems amply to justify the present, what could the future do better than continue in the way of both? There is no important step to be reconsidered, to empirical science, Empirical philosophy offers only the encouraging counsel "Perge modo et qua te ducit via dirige gressum "

And yet the future proved not content with this comfortable philosophy Had it not indeed taken itself too comfortably? Had it, for example, scrutinized as closely as it should those "bare facts" that presented themselves as the beginnings of knowledge? Was there indeed nothing suspicious about them save the names by which they announced themselves? "Announced themselves", we say, for the Empiricist's account of their reception mentions no one who could have served as their announcer Later on, teachers taught us many things, but from what untaught beginnings? At all events, if these "simple ideas" were too simple to announce themselves, then they must have come to us unannounced And yet we are supposed to have recognized them at once "This is *white*, this *hard*, this *sweet*!" It would have been no wonder had we made mistakes, the wonder would have been, had we been able to make mistakes How could we have managed to put an incoming idea in the wrong box, when we had no boxes in which to put any ideas? We had, the Empiricist assures us, no general ideas under which to subsume them neither *white*, *hard*, *sweet*, nor yet *quale*, *quale*, *quale* The mind was "as we say, white paper, void of all characters, without any ideas "

Here it is useless to answer, The "particular sensible

object" affecting us *was* white, though only later did we come to recognize its whiteness If it was not white for us when first it came, then what was it for us? Perhaps we begin to catch the later spirit in which Immanuel Kant answered, It must have been for us "so viel als gar nichts " What we can recognize as nothing is for us no more than nothing Or if, as a later Empiricist puts it, "a thing is what it is known as," then what is known as nothing can be but nothing

But you may say, we do constantly distinguish between what an episode means to the reflective onlooker and what it is for the one immediately experiencing it "A rag, a bone and a hank of hair" she may be *for us*, yet *he* will call her "his lady fair " Are not the mature philosopher and the new experiencing child in some such relation as this? To the philosopher, the true description, to the child, the immediate experience?

We answer To put it this way is to miss the whole point of the joke This lay, not in our knowing madonna for what she is, and his knowing her *not at all*, but in our taking her for what she is, and his taking her for something else But to be something else is still to be something, it is still to fall under some concept, though the wrong one The predicament in which our new "white paper" mind finds itself seems hopeless, called upon to know what is being imprinted on it, it has as yet *no* concepts, not even that of *something*, under which to subsume whatever is not *nothing* For the Empiricist, all concepts are come by through experience, including the one that Kant came to call *der Begriff eines Objects überhaupt*—the concept, as one might say, of a *something or other* No wonder if the pass to which Empiricism, allowed to go its own way, had unwittingly brought itself, seemed to Kant "critical "

Nor was this difficulty of accounting for our first recognitions the only one to which Empiricism could furnish no solution Criticism raised a second question, also concerning the beginnings of experience, but revealing a new and independent source of embarrassment If our first recognition of simple ideas "presupposes" (this was Kant's favorite word) one kind of equipment, does not our first acquisition of complex ideas presuppose another kind?

Assuming that we were somehow possessed of general concepts by which our new mind could recognize its first guests, the next task Empiricism set us in learning the world was to observe which of these guests habitually called in company To this, their habit of coming together, as also to their other habit of coming in regular sequences, we owe our knowledge of objects and their laws, i e , the whole complex world of our experience

We are, namely, to observe what "simple ideas exist in several combinations united together " Evidently we could not do this had we not some idea of what sort of experience should be recognized as an "existing together" of *white*, *sweet*, *hard* (as in a lump of sugar), what other sort would be an existing of *white* apart from *sweet* and *hard* (as in a flake of snow) Some acquaintance with *togetherness* and *apartness* is surely presupposed by our ability to note what qualities are met with "existing together" and what "existing apart"

Consider this idea of togetherness *All* our ideas are said to be either simple or complex If so, it must be a simple idea either of sensation or of reflection Now the union of *white*, *sweet*, *hard* in a lump of sugar is surely not a *white*, *sweet*, *hard* union! Again, this union may well be perceived as an "act of the mind" by which it is perceived, it is not, though, a *simple* idea of reflection Not to elaborate the

obvious, the idea of *togetherness* is to be found neither
among the simple ideas of sensation, nor among those of
reflection, which two classes nevertheless exhaust all our
simple ideas If not simple, is the idea of *togetherness* to be
found among our complex ideas? It would be idle to search,
for was it not by observing what simple ideas occurred
together that we came by these complex ideas? How could
we have acquired, as a result of observing, an idea without
which we should not have known what to observe?

Perhaps you will say, Whatever the specious cogency of
the preceding reasoning, we certainly must be able to
observe what qualities occur *in the same place at the same
time* Is not an ability to observe such space time coexistence
of "simple ideas in several combinations united together"
sufficient for the purpose of acquiring complex ideas? To be
sure it might be, had we this ability to start with, but one of
Empiricism's most valuable contributions to psychology was
a study, first, of space perception, then later, of time
perception It is to Berkeley, in his *New Theory of Vision*,
that we owe the first shrewd analysis of how we come by
our perception of space relations We may leave as a foot-
note Berkeley's own account of *how* our ability to locate
positions in space and time is come by,[4] the admission (by
him and all later psychologists) that it *is* come by is enough to
render coincidence in space and time unavailable for
furnishing that idea of *togetherness* presupposed by our
ability to come by any complex knowledge whatever,
including of course the complex knowledge of *togetherness*

But with this note on Berkeley's contribution have we
exhausted all the ways in which Empiricism might account
for a "white-paper" mind having an *unacquired ability* to
recognize the occurrence together of some simple ideas, and
not of others, whereby it might acquire, in time, all "that

vast store of complex ideas which the busy and boundless
fancy of man has painted on it . ."? May it not be that,
although the idea of space-time togetherness is come by
only through experience, yet the ability to observe that
certain simple ideas occur "in combinations united to-
gether" as attributes of the same individual subject is
altogether primitive and unacquired? Only let us note that
this *individual subject*, then another, then another, has the
attributes of *white, sweet, hard,* and should we not after-
while come by the general notion of the class of individuals
called "lumps of sugar"?

Locke, we can now see, struggled hard to grasp the matter
in just this way The result, even in the eyes of Berkeley
and Hume, was the most unhappy failure of his *Essay* For
what makes an individual? Or, what individuates the
universal or abstract qualities, *white, sweet, hard*? What
makes one lump of sugar different from another which has
(as it may well have) exactly the same qualities? Locke's
answer is historic

> "The mind being furnished with a great number of
> simple ideas, takes notice also that as certain numbers
> of these simple ideas go constantly together which be-
> ing presumed to belong to one thing are called so
> united in one subject by one name And, not imagining
> how these simple ideas can subsist by themselves, we
> accustom ourselves to suppose some *substratum* wherein
> they do subsist, which therefore we call substance "7

Now if the mind at the outset of its learning were pro-
vided with a primitive idea of *substance*, all would yet be
well with Empiricism "Occurring together" would mean
"observed to inhere in the same substance," "occurring

apart" would mean "observed to inhere in different sub-
stances." We might then acquire at our leisure the ability
to locate these occurrences in space and time. But *does*
Locke suppose the mind to have this *a priori* equipment?
Far from it: the idea of *substance*, so far from belonging to us
at the outset is never acquired at all! For

> "If anyone will enquire for himself concerning the
> *notion of pure substance in general,* he will find he has
> no other idea of it at all but only a supposition of he
> knows not what support of such qualities which are
> capable of producing simple ideas in us. If anyone
> should be asked, What is the subject wherein color
> and weight inheres, he would have nothing to say, but
> the solid extended parts. And if he were demanded
> what is it that the solidity and extension inhere in, he
> would not be in a much better case than [that] Indian
> who, saying that the world was supported by a great
> elephant, was asked what the elephant rested on? To
> which his answer was, a great tortoise. But being asked
> against what gave support to the broad-backed tortoise,
> replied, something, he knew not what. And thus here
> we talk like children, who being questioned what such a
> thing is which they know not, readily gave this satis-
> factory answer, that it is *something;* which in truth
> signifies no more when so used either by children or
> men but that the thing they pretend to know and talk
> of is what they have no *idea* of at all, and so are perfect-
> ly ignorant of it and in the dark."[8]

Any way the matter is put, then, the Empiricist seems to
have assumed (without being aware that he had assumed
it) a double equipment on the part of the beginner on life's
ways: (1) an ability to distinguish between *something* and

nothing, (2) an ability to distinguish *this* thing from *that* thing The two abilities are plainly different in kind, the general concepts that make recognition possible cannot help us to individuate (i e , to grasp what is just *not general* to any two things, however like), the knowledge that we are dealing with a single individual does not inform us of its kind (i e of what is *not singular* to it) But however unlike the two equipments, it would seem impossible for one who would make a start on the road of learning to dispense with either

All of which amounts to saying, However sound may be the Empiricist's account of how our knowledge has grown, once having started, his own account of this growth makes it impossible for its start to be part of it i e , a something grown "Experience" is a name for our intellectual stride, it does not include in its meaning the foot-hold from which the stride swings off Must not, then, this beginning be given from without, is it not a "presupposition of learning," not itself learned?

Before we are in any position to judge the hardness and fastness of the world of facts as experience reveals it to us, we must examine the nature of what is called the *"Bedingungen die die Ehrfahrung moglich machen,"* the conditions that make experience possible

To search for the presuppositions that make learning-by-experience possible?—this was what Kant seized upon as the first task of a Critical Philosophy

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[1] *Essay Concerning Human Understanding,* 1, 1, Sec 2 and 8
[2] *Essay,* II, i, 3–5, selected
[3] *Essay* II, xii, 1, arranged
[4] *Essay* II, xii, 2
[5] Helmholtz, *Physiologische Optik,* 2A, 610f 'nur die Qualitaten der Empfindung als wirkliche Empfindungen zu betrachten sind "

*The general trend of the "New Theory of Vision" may be indicated by a few of Berkeley's reflections of our perception of distance —

11 It is plain that distance is in its own nature imperceptible, and yet it is perceived by sight It remains, therefore, that it be brought into view by means of some other idea that is itself immediately perceived in the act of vision

16 It being (thus) shown that distance is *suggested* to the mind by the mediation of some other idea which is itself perceived in the act of seeing, it remains to enquire *what* ideas or sensations there be that attend vision unto which we may suppose the ideas of distance are connected

And, first, it is certain by experience that when we look at a near object with both eyes, according as it approaches or recedes from us we alter the disposition of our eyes by lessening or widening the interval between the pupils This disposition of the eyes is attended with a sensation which seems to me to be that which in this case brings the idea of greater or lesser distance into the mind

17 Not that there is any natural or necessary connexion between the sensation we perceive by the turn of the eyes and greater or lesser distance But—because the mind has by constant experience found the different sensations corresponding to the different dispositions of the eyes to be attended each with a different degree of distance in the object—there has grown to be an habitual or customary connexion between these two sets of ideas

20 From all which it follows that the judgment we make of the distance of an object viewed with both eyes is entirely the result of experience

45 And I believe whoever will look narrowly into his own thoughts and examine what he means by saying he sees this or that thing at a distance will agree with me that what he sees only *suggests* to his understanding that often having passed a certain distance [to be *measured* by the *motion of his body* which is perceivable by sense] he shall come to perceive such and such tangible ideas which have been usually connected with such and such visible ideas

    Essay II, xxiii, 1
    Essay, 2, 25   2

*The present study finds that to translate Kant's *Erfahrung* by the participle *experiencing*, or *learning by experience*, renders the sense in which Kant uses the term better than the usual translation *experience*

4. The Mind's Lawmaking: Critical Idealism

PLEASED WITH SOMETHING DRAMATIC IN THE PHRASE, we often characterize experiment as "a question put to Nature " But if there be anything flattering to our state in Nature's submission to our questioning, there can be nothing grateful to our heart in her manner of answering "Are we all to die?" Nature answers, "Yes " "Will the sun that warmed our fathers last out our children?" Nature answers, "No " Thus Nature giveth and Nature taketh away, her complaisance ends with letting us know her way with us

Without comment, let these commonplace reflections be followed by a Kantian sentence of the year 1781

"The order and uniformity in the phenomena we call Nature we ourselves bring into them and never had we found them there had we not first put them there "[1]

A few years later (1787) Kant likened the "revolution" his own world-view had undergone to a "Copernican" change of standpoint [2] The phrase is just, it catches and accounts for that sense of disorientation which seizes on anyone trying for the first time to follow the swing of Kant's thought from Rationalism and Empiricism to a manner of thought that has come to be called Criticism The phrase is just, but it would have been more faithful to history had it added a clause "Copernican" in magnitude and importance Kant's change of viewpoint was, in sign and import it was exactly the opposite Copernicus had come into a world centred in and revolving around man's dwelling place, the Earth, his *De revolutionibus* has left man remotely swung

along on some vague satellite of a central orb, the Sun The
first Kritik found experience informing man's curiosity of
indifferent Nature's way with him, it left man in forming
Nature with the laws of his own understanding

His contemporary, Moses Mendelssohn, thought fit to
refer to Kant as "der Allzmalmer," which might be done
into "the Allshatterer " Yet this thorough going shatterer
of all older philosophies was in his social demeanor as staid
and decorous a citizen as you please His way in life lay
through the quiet streets of Königsberg, from house to class,
from class to house, "and the neighbors knew it was exactly
half-past three when Immanuel Kant, in his grey coat, his
malacca cane in hand, left his door and went his way to the
lime tree avenue which is still called in memory of him The
Philosopher's Walk " (Heine) All men trusted, colleagues
consulted, students revered, friends loved "den Alten von
Königsberg " His King had in him a dutiful subject, for
when the King's government called attention to some
objectionable points in the professor's theology, Kant in
conscience bound stopped professing these points—pending
a change of government

If it has been thought relevant to recall Kant clothed in
all the sanctities he wore in his neighbors' eyes, it is with
the thought that one may come to see that the mind of this
All shatterer was no less controlled than his manner of life
In the sense in which Macaulay could say of the Copernican
revolution that it must have come about had there been no
Copernicus, one may come to feel of the Kantian revolution
that it must have come about had there been no Kant
Recalling the conclusions of the last two sections, one may
well feel that some later thinkers must have come to see, as
Kant, (1) that Rationalism had proven empty, (2) that
Empiricism had been born blind

However, as history would have it, it was Immanuel
Kant who first thought his way through the criticisms whose
inevitable course we followed in the last two sections to this
same double conclusion Questioning, gradually gathering
objection, is the burden of Kant's "Precritical" writings,
which bring his story down almost to the year 1770 In that
year his *Inaugural Dissertation* reveals him done with the
old, if only half-ready with the new The unfinished half of
this new method absorbs and baffles him for the "ten years
of silence" that follow It was an all but sixty-year old
revolutionary who wrote the sentence quoted at the begin-
ning of this chapter Let us return to that

"The order and uniformity in the phenomena we call
Nature, we ourselves bring into it!" But one who would
take on himself any responsibility for making this surprising
statement seem natural and inevitable to a mind reflecting
on the "emptiness" of Rationalism and the "blindness" of
Empiricism, should hasten to begin by relieving Kant's
words of a burden they seem to, but do not, assume

It was not without purpose that the Kantian utterance of
this chapter's second paragraph was left, without comment,
in such close juxtaposition with the sane sentiments of its
first paragraph Could any uninitiated reader have escaped
the impression that the laws Kant supposed us to bring
into Nature were just such of Nature's laws as had imme-
diately before been adduced, laws of biological death, laws
of cosmic destruction? And is it we ourselves who bring
such ordinances into Nature? But this is preposterous, is it
not? In the first place, how could we? In the second, why
should we? They are such inhuman laws

Kant was in enduring fear lest some such hopeless para-
dox as this be taken for his doctrine And since history has
done what it could to justify his anxiety on this score, it is

well we should part with this interpretation first, lest we
depart with it at last as something of Kant's own For Kant
is never tired of warning the reader that he has no such
meaning, he knows too well the part played by hard fact in
informing our experience "We do indeed [Kant writes]
learn many laws from experience "[3] and again, "There are
many laws of Nature we can only know through experi-
ence "[4] So that, if there are any laws "we ourselves, or the
nature of our mind (Gemuth) bring into phenomena" they
cannot be such laws as our opening paragraph cited, for
only experience, hard experience, forces us to accept these
laws, and "of course empirical laws as such can no more
derive their content from the mere understanding than can
the measureless variety of phenomena in space be grasped
from the mere notion of space "[5]

But if such unity and uniformity as the nature of our
human understanding may have introduced into the
phenomena we call Nature is reflected in none of those laws
which empirical science shall find to fit the brute facts of
experience, in what aspect of these phenomena is this unity
to be looked for?

Had all the findings of Hume's *Enquiry* remained as
consistent with the fundamental thesis of Empiricism as did
his final dismissal of *substance* (*spiritual* no less than
material) from the meaningful terms of discourse, Hume
must have answered our question with a simple, *In none*
Later Empiricists (notably Hume's critical admirer, Hux-
ley), having detected and avoided what inconsistencies
remain in Hume, did so answer [6] In no aspect of the pheno-
mena we call Nature is any uniformity to be looked for
other than such as is to be found in those empirical science
finds fitted to the bare facts of our observation But, radical
Empiricism having gone so far, cannot escape going a step

further; its final reflection must take on some such form as
this: "Although we are unlucky in finding so many distress-
ing laws in Nature, are we not lucky to have found any law
there at all? We might have found her quite lawless and
chaotic." Not so Kant: "there are many laws of Nature [we
have heard him say] that we can know only through
experience;" but that there is law connecting phenomena
—this we can learn through no experience. And this is the
whole matter between Kant and the Empiricists. For the
Empiricists, we know there is law only because we find laws.
For Kant, we do indeed find what laws there are; but we
know *a priori* that law is there.

How, you ask, can we know this? "Because [I complete
the last quotation] learning by experience itself depends
on such law as a condition of its own possibility." And if one
would know finally in what sense our ability to find *laws*
depends on our assurance that there is *law*, Kant's answer
comes to this: Experience (and so Nature as revealed by it)
is not the name of an aggregate of bare facts, each intelli-
gible by itself and only accidentally found to be connected
by certain laws. The maximum isolation into which a fact
of experience can fall is incomplete. It must have been
accorded a place in some observable system of facts, else it
would not be sufficiently understood to be so much as an
object for observation. Wherefore, "although we learn
many laws from experience, yet these are but particular
cases falling under higher laws, of which the highest come
a priori from [the conditions of our] understanding. These
are not gathered from experience, but rather insure to
phenomena their law-abiding character, and just in that
way make experience [i.e. this 'gathering from experience']
possible." And the passage closes with a return to the
original theme: "Understanding is not merely the function

of formulating laws after comparing phenomena, it is itself
law-making for Nature   Apart from understanding there
would be no Nature "[7]

We cannot have found by looking for them the eyes
without which we could not have looked Such, in homely
metaphor, is the contention of the preceding paragraph,
such is the eternal contention of Criticism And the con-
tention cannot be called an unreasonable one, if it can first
be demonstrated that the empirical search after facts and
their laws does presuppose an initial (*a priori*) equipment
But to this very conclusion the attempt of our last chapter
to grasp what Empiricism was trying to say about itself led
us steadily on We saw there that one who brought nothing
to experience could learn nothing from it, *and that this must
be as true of our first experience as of our last* Whatever we
find prerequisite to the making of any intelligible experi-
ment, that is what Kant means by the mind's *a priori*
equipment, or, what he came to call the mind's *a priori
forms*

What, then, is the equipment prerequisite to all observ-
ing? As we let classic Empiricism tell its own story, we saw
that it did not, (because it could not), tell the whole story
of the learner by experience It always assumed him
possessed of two things, but how he came by them it never
told One was an ability to recognize his first guests in the
way of ideas, the other, an ability to tell one guest from
another of the same likeness The first presupposed an *a
priori* equipment in the way of a provision-for-recogniz-
ing, the second, an *a priori* equipment in the way of a
provision-for-individuating

Reflecting on these shortcomings of the Empiricist
philosophy, Kant could come to no other conclusion than
that to which we ourselves were led If he was painfully

slow in arriving, that is because he had first to find the way,
our critical thoughts of the last chapter really followed a
course later history had taught them to anticipate Then,
too, these thoughts came to no more than a general recogni-
tion of two kinds of presupposition that certain historic
Empiricists had unwittingly made Kant must assure him-
self not only that these presuppositions *had been* made, but
also that they *had to be* made, only then would he feel
driven to restate the meaning of experience Finally, he
must know not merely *that* provision for individuating was
a prerequisite to observing, but also *what* provision, and
again, he had to know not only *that*, but also *what* concepts
made recognition possible

In a previous paragraph we referred to the Kant of 1770
as having been "half ready" with his Critical theory of
experience The address which he read on the occasion of his
inauguration as Ordentlicher Professor shows him to have
come to a decision, one from which he never afterward
departed for all percipient beings the only possible way of
individuating is by the use of *space time coordinates* The
first book, "Aesthetik", of the *Kritik der reinen Vernunft*
does no more than repeat somewhat hurriedly and care-
lessly (as though the matter had exhausted its interest for
Kant) the presentation of the inaugural dissertation, *De
mundi sensibilis atque intelligibilis forma et principiis*

It has often been asked whether in accepting the space
time method of individuation Kant had sufficiently con-
sidered all the possibilities of the case It cannot be said that
he did more or saw more to do in this way then just one
thing So far as perceptual objects are concerned, the
doctrine of space time individuation had known but one
serious rival namely, the doctrine of *identification by minute
description* Receiving its clearest formulation at the hands

of Leibnitz[8] it had really been implicit in all Rationalism
and is essential to the philosophy of that school. Beginning
his reflective life under the influence of Rationalism, only
slowly and painfully emancipating himself from its funda-
mental doctrines, finally rejecting them, Kant places himself
in opposition to the undisputed authority of his day.

At this distance we may see that Kant had both a richer
tradition and a more practical sanction on his side of the
argument. As for the tradition, it goes back at least as far as
to Plato, who in his *Timaeus*[9] makes space the "pure matter"
that individuates general qualities (i.e., takes care of the
distinction between *this* thing and *that other* precisely like
thing). And from Plato's time down, it would be possible to
trace an imposing history of space-time individuation. As
for practical sanctions, our courts of justice recognize the all
important difference between the evidence established by
"identification by minute description" and the evidence
established by "individuation by space-time coordinates."
The former, Leibnitzian, method of identification is exactly
the one underlying our present method of "police identifica-
tion." With sufficient refinement of detail, Bertillon
measurements may be made to constitute an indefinitely
minute description of the *kind* of man a certain individual is.
But suppose the accused "identified" with the culprit to the
limit of available description; how long would he remain in
the dock could he establish an "alibi"? In insisting that the
same individual cannot be in two places at the same time,
in admitting that two individuals, however like, can be,
the law throws the whole weight of its authority on the side
of Kant and against Leibnitz.

Thus our valuation of the past and our practice in the
present alike engage our sympathy on Kant's side in this
historic conflict between descriptive "identification" and

space-time "individuation" And if no third alternative
presents itself to our mind (as to Kant's none did) then,
having already convinced ourselves that some means of
individuation is presupposed by our ability to learn from
experience, the conclusion is forced

(1) The barest facts given to the observer must already
have received the "form" of a space-time arrangement, and
as there is nothing common to all possible experiences save
the observer and the observed, what is not given *to* the
observer, must be given *by* him Whence (2) the space-time
form in which facts are found ordered must have been given
to them by the perceiving mind, and, since the only known
way of arranging objects in a space-time order is by con-
structing geometrical (i e , space-time) coordinates, it follows
that (3) geometry is a science imposed on, not learned from,
the phenomena of experience It is an *a priori* science, and
all its propositions are *a priori truths*

Now, as we approach the problem of the second book of
the Kritik, the "Analytic", we might suppose the hardest
part of our discussion to lie before us It is the problem of
what we called the equipment for recognition, and we
remember Kant's ten-years of silent quest for this *a priori*
equipment Through these years he sought what when
found he called the "guiding thread " But it was in the
quest for this thread, not in the following of its guidance
to a final conclusion that the years passed And so, for us,
the way having been found, the end to which it leads is
before us almost before we know it

Indeed, what for Kant was the great revelation is already
before us In those paragraphs in which we sought to set in
clear view the effects of the "Copernican revolution", we
were led to realize a certain new dependence of Nature's
laws on the "law of understanding " Facts must conform to

this law before they can be observed, they must let themselves be understood in some way if they are to make themselves known in any way Such was Kant's whole contention

But how was this "law of understanding" to be found and defined? Kant's labors were near their end when in old-fashioned logic he came upon the "guiding thread" that would lead him directly to "the discovery of all the pure (i e non-empirical) concepts of the understanding" And our labors to that end should be even less taxing than were Kant's own, unpracticed as he was in using his own discovery

Logic may well enough remain for us what it has been in all tradition, the "science of reasoning", but in order to serve the purposes of reasoning (for example, the deduction of the theorems of geometry from its axioms) logic has first to be understood Now, as we know, the propositions that logic accepts as valid are composed of substantive symbols (a, b, c, etc) connected by expressions of relation (All is, If-then, Either-or, etc) These propositions purport to be valid whatever substantives the a, b, c,   may stand for, but no one of them, valid for one relation, is valid for all How arrive at an understanding of the relations that will let us recognize the difference between any two of them?

Elementary logic had from the beginning faced the task of making these relations understood just as any other science would approach the definition of its concepts A "quadrangle" is understood when it is included in the genus "figure" and distinguished from other species of that genus by the differentiae "four sided" and "plane " Just so the logician proceeds, his science places among the propositions valid for all substantives, such fundamental ones as *All a is a*, *No a is non-a*, *Either all a is b, or some a is not b*, *If all a is b, then some b is a, etc* Here the expressions of relation

are *All-is, No-is, Some-is, Some-is not, Either-or, If-then*
The logician recognizes all these as "logical relations", since
they enter into propositions meant to remain valid for all
substantives And there is a strictly limited number of
relations that can so enter If our logic is sound, any attempt
to increase the number of proposition-forms, distinguished
by the different relations that enter into their several word-
ings, must meet with failure For instance, it might suggest
itself that *a equals a* is as valid for all values of *a*, as is *All
a is a* But though we use the same substantive-symbols in
both, we do not mean to give the same unrestricted mean-
ing to those of the first as to those of the second formula
We do not mean to say that *perpendicularity = perpendi-
cularity*, and we may well mean not to say that *infinity =
infinity* But if we were not prepared to say that *All
perpendicularity is perpendicularity* and *All infinity is
infinity* we could not admit the terms, *perpendicularity,
infinity*, to our scientific vocabulary For, if proposition-
forms that should not lose their validity so long as their
term-symbols (*a, b, c,*   ) stood for *anything*, did not
remain valid when *perpendicularity* or *infinity* was sub-
stituted for one of these symbols, then either these terms
stand for nothing or propositions of the form *All a is a* do not
conform to "the conditions of our understanding"

Having distinguished logical relations from all others that
can enter into our discourse, the logician has only to dif-
ferentiate each of these relations from the rest and he will
have defined the "concepts of logic" To this end he collects
enough and just enough adjectives to serve as differentiae
The student of elementary logic is made painfully familiar
with these adjectives The categorical forms using *All-is,
No-is, Some-is, Some-is not* are distinguished from the
hypothetical, using *If-then*, and from the *disjunctive*, using

Either-or Among categorical forms, the *universal affirma-
tive*, in *All is*, is distinguished from the *particular affirma-
tive*, in *Some-is*, from the *universal negative*, in *No is*, and
so on One need revive no more elementary memories,
enough to remark that if we used the text book of Kant's
day, our list would be complete with just twelve such
adjectives [10] They, or rather the abstract substantives whose
adjectives they are, *universality*, *affirmation*, etc , would
constitute the "concepts of logic " They are, or should be,
Kant's "categories of understanding", though as a matter
of fact, Kant's translation of the adjectives of his "table of
judgments (proposition-forms)" into the substantives of his
"table of categories (concepts of the understanding)"
enriches the meaning of the substantives far beyond the
limits strict translation justifies But for the moment, our
interest lies in grasping the intentions of Kant, not in
judging his success in realizing these intentions

  All this of course is perfectly traditional, any text-book
would give some such list of concepts and would insist that
an understanding of them was a prerequisite to the use of
logic in reasoning But Kant contends for more, for much
more These concepts are the indispensable equipment of
any mind that would *understand anything*, without them it
could not even enjoy meaning enough to be mistaken It
could recognize nothing, not even that most general of all
"things", which Kant calls an "Objekt uberhaupt " All its
moments of experience would be for it "so viel als gar
nichts " *It could gather no experience*

  Nor is it hard to catch Kant's point To grasp a meaning is
to define, to define is to classify, to classify is to subsume, to
exclude, etc How much of the meaning of *square* would I
gather from the definition "All squares are, etc " did I not
know what the logical relation *All-is* meant? But, as we

have seen, an understanding of what *All-is* means pre-
supposes the possession of the categories, those pure concepts
of understanding

We need not labor the point Evidently what we have
said of the prerequisites of grasping the meaning of, or
recognizing, a "square" would hold all its force, if for *square*
we substituted *white, sweet, hard,* or *quale, quale, quale* or
just something (= *Objekt uberhaupt*). The conclusion is
forced and may be worded in a way quite analogous to the
way in which the conclusion of the *Aesthetik* was worded
(1) The barest facts given to and recognized by the observer
must have fallen into some classification meeting the de-
mands of logic, and as there is nothing common to all pos-
sible observations of fact save the observer and the observed,
what is not given *to* the observer must be given *by* him
Whence, (2) the first, most general classification in which
facts are found ordered must have been given to them by
the observing mind and, since the only way in which a
classification-frame can be constructed in which all kinds of
facts can be ordered into mutually exclusive and collectively
exhaustive classes is by the use of formal logic, it follows
that (3) logic is a science imposed on, not learned from, the
phenomena of experience

It is in imposing this logical "order and uniformity on the
phenomena we call Nature", that the mind assures them a
law-abiding character, whatever these phenomena may be
With this, it remains for the mind to learn by experience by
what laws these phenomena abide Without this, the mind
could not so much as learn by experience that phenomena
were lawless A world whose phenomena presented them-
selves as a *Gewuhl der Erscheinungen*[11] (chaos of appear-
ances) or a *Rhapsodie der Empfindungen*[12] (rhapsody of
sensations) could not even be known to be such a world, or

to be any sort of a world, or to hold any sort of a thing that might be called a fact

It will be noticed that no attempt has been made to represent the mind's *a priori* equipment as simple Neither the geometry by which it individuates nor the logic by which it recognizes are such as a babe might know It is not pretended that the forms of mind required for all learning are acquired by a little All the more anxiously then do we ask, How is it possible for the mind to possess *a priori* science and to impose it on "the phenomena we call Nature"? For Kant this was the most important problem of all philosophy and "and all metaphysicians are hereby formally and officially suspended from their functions until they shall have satisfactorily answered the [critical] question How are synthetic [e g , not definition-wording] judgments *a priori* possible?"[13] Whatever difficulties we may finally realize in accepting, or even comprehending Kant's answer, we may at least invent or sort of a parable that will give some idea of the motives underlying Kant's answer

A parable Suppose a wine-glass were to ask itself, How is it that whatever the color, flavor, bouquet of the wine poured into me, my contents have always the same contour? One answer that might very naturally occur to this reflective wine-glass would be It must be that all properties save the form of the material given me, are determined from without and depend in no wise on what I am Whereas the form which never changes, no matter where I go or what I do, can only be impressed *on this content by my own constitution and nature* Just so Kant, having convinced himself that every being capable of having experience at all must find this experience arranged (1) in a space-time order, (2) under laws of the understanding, concludes that the subject himself or the nature of his mind must impose

these forms upon whatever is offered him in the way of sensuous material. Thus do we bring geometry and logic "into the phenomena we call Nature."

Any philosophy may fairly be called "idealistic" which recognizes that the subject contributes something of his own nature to the making of the real world. If then Kant has been led to see that this very real world we call the world of experimental facts has certain important aspects which we ourselves impose upon it, aspects which indeed "we had not found there, had we not first put them there", it is quite true to say that our search for the "inexorable laws" which Rationalism, and "hard facts" which Empiricism held out to us as the very stuff of reality, has carried us into Idealism. It is such Critical Idealism as would maintain that however inexorable laws, and however hard facts may appear, they owe what is most inexorable and hardest about them to the way we have made them. The outcome is at least curious, but before we can estimate its bearing on our original question, What obstacles does reality offer to human desire?—we must face much more seriously than we have yet done the question, In what literal, non-figurative sense can the mind have a "form" that it imposes upon the "material" of sense-experience?

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[1] *Kritik der reinen Vernunft*, A 125 abridged
[2] *Ibid.*, B 15 and 22 (note)
[3] *Ibid.*, A 126
[4] *Prolegomena*, A 111
[5] *K d r V*, A 127
[6] Hume's inconsistency with the fundamental thesis of Empiricism is clearly evidenced in the sentences which introduce Section II of the *Enquiry* "All the objects of human reason or enquiry may naturally be divided into two kinds, to wit, *Relations of Ideas*, and *Matters of Fact*

"Of the first kind are the sciences of Geometry, Algebra, and Arithmetic, and in short, every affirmation which is either intuitively or

demonstratively certain   Propositions of this kind are discoverable by the mere operation of thought, without dependence on what is any where existent in the universe

"Matters of fact, which are the second objects of human reason are not ascertained in the same manner, nor is our evidence of their truth, however great, of a like nature with the foregoing   This proposi tion, *that causes and effects are discoverable, not by reason but by experience,* will readily be admitted with regard to such objects, as we remember to have once been altogether unknown to us   '

Hume's argument goes on to show that what is generally admitted in the particular case must, on reflection, be seen to hold in all cases in which we assert that a given event follows from such and such a cause, or is followed by such and such an effect

Huxley's criticism of the first of these propositions, recognizing the non empirical nature of our acceptance of the theorems of mathematics, is to be found in the early paragraphs of his *Essay on Hume,* Chapter VI

That the ultimate reflections which we have put into the thoughts of the radical Empiricist do not materially depart from those which this radical has himself put into words is sufficiently established in the follow ing quotation from the same chapter of Huxley

' The axiom of causation [' Every event has a cause '] cannot possibly be deduced from any general proposition which simply embodies experience   But it does not follow that the belief, or expectation, expressed by the axiom, is not a product of experience, generally antecedently to, and altogether independently of, the logically indefen sible language in which we express it "

⁷*K d r V,* A 126

⁸Leibnitz called it the *principium identitatis indiscernibilium*

⁹*Timaeus,* 52

¹⁰A slight departure of Kant s formal logic from the traditional texts, being of no interest to later logic or to the present study, is passed over

¹¹*K d r V*

¹²*Ibid*

¹³*Prolegomena,* A 44

5. A Priori Science: A Paradox

THE SUGGESTION MAY WELL SEEM STARTLING, THAT we never find a fact to have been observed and reported by an observer but that we also find the observing mind to have contributed something of its own nature to the account it gives of this fact.

True, nothing is more familiar to us than the developed mind's habit of interpreting what is newly set before it in the light of what it has already taken into its make-up. Consider but the conflicting opinions of those who would pass judgment on a new book, a new painting, the last pronouncement of the Administration; it is generally easy, often amusing, to make out the pre-judgments on which such judgments rest. "He could see things in no other way", we say of such and such a critic, quite meaning that our critic is bound to review things in the light of his own prejudices. Studying his reactions, we make out the mould of his mind; knowing the cast of his thought, we foresee his reactions. But where we take the conditions forerunning and shaping judgment to be personal or class-prejudices, we have no difficulty in accounting for them as acquired, whether gathered from the experience of the individual or born of the experiences of his race. With race and circumstance they vary, and we are accustomed to think a somewhat broader experience, a less hide-bound imagination, a deeper reflection, would rid a self-critical soul of the more pernicious of his prejudices.

What Kant proposes is quite otherwise difficult to accept:

it is the theory that *certain* preconceptions and *certain* prejudgments are not acquired at all, since neither the individual nor the race lacking them could so much as begin the acquiring process we call "learning-by-experience" They are therefore "universal and necessary" to all who learn, and as such must be furnished by the mind itself, not by the events befalling it Since this is so, the part played by the mind's own nature in shaping the world it experiences must be the same in all men The events befalling each individual may be as unpredictable, inexplicable, random as you please, they must yet exhibit the measure of unity and uniformity without which the mind could recognize none of them as facts of Nature

There are two injustices we may do this or indeed any other revolutionary moment in history we may not have spent our best imagination on thinking ourselves sympathetically into its motives, or, having done this only too well, we may not have had energy left to think our way out of its conclusion It is not too much to say that Kant along with other revolutionaries has suffered more from the second than from the first injustice, for a revolutionary never wins more than part of his cause, never proves to have been more than half right, and those do the least justice to reformations who struggle not to let them be reformed The assurance that we are not at all likely long to abide in Kant's new world ought to make it all the easier to let ourselves go to the thought that brought it into being

Empiricism, we saw on examining it, had failed to keep its promise to show how all the ideas with which the mind is furnished might be come by through experience The idea of occurring together, which is at bottom the idea of individuality, was not set forth as derived from experience, but was left an unsuspected presupposition of any such

deriving The meaning of certain class-names (white, hard,
sweet, etc) under some one of which a first experience
must have been subsumed before it could be called by any
given name, were not allowed to be won from experience,
but were tacitly assumed to be already at the disposal of the
mind whose very first impressions were "recognized" as
being, this one, a sensation of *white*, that one, of *sweet*, etc

And now Criticism has made it clear that this failure of
Empiricism was no such oversight, negligence, or want of
skill as a more careful working over of the doctrine might
correct It has shown that experience is from beginning to
end such an interplay of subject and object as to make it
inevitable that he who brings nothing to the game can gain
nothing from it The least with which the adventurer may
be armed for a profitable struggle with the world—whatever
the world in which his lot has cast him—is a two-fold
equipment a provision for recognizing whatever may be-
fall him as at least a something-or-other, and a provision for
distinguishing *this* something from *that* other similar some-
thing, before he can begin learning anything

If one ask, How is this possible? How can one start on a
career of discovery with something already discovered?
There can be but one answer within the range of possibili-
ties so far exploited There are conceived to be but two
parties to the game of experience the observer and the fact
If the fact does not furnish the *a priori* knowledge, the
observer must, it must be given to him as part of himself,
provided for him by the "Nature of his mind " His "apper-
ceiving" mind endowed with a form to which all that comes
its way must conform, if it is to be perceived at all, the
traveler along life's ways finds in the Nature about him a
reflection of the nature of his own mind It is on recognizing
this that one becomes an Idealist

The name *idealist* readily suggests one who has, in whatever way, succeeded in emancipating himself from the tryanny of fact, or has (as those that love him not would say) lost his hold on reality It is easy to see that our Critical Idealist has so far earned a right neither to the sympathies nor to the antipathies that the mere name *idealism* is likely to inspire Nothing he has so far proposed in the way of a theory of knowledge suggests a softening of the hard outlines in which his predecessors, whether of the Rationalist or of the Empiricist persuasion, had blocked out the world of reality Logic and geometry are just as surely forced on the Kantian idealist as on the Cartesian realist, with only this difference that Kant would represent us as forcing them in our turn on facts, while the Rationalist failed (though in his "ontological proof" he tried) to show how any application to fact could be assured them Facts, on the other hand, are no less solid for Kant than for Locke, the only difference being that Kant will have us to contribute something of our own hard nature to their solidity

But whether pleased with this outcome or disappointed by it, one who has grasped so much of the significance of the Critical Philosophy will find it hard to escape a sense of its finality It seems to have caught up all causes of failure in previous systems and to have made similar failures impossible for the future, and it seems to have done this in the only possible way Certain it is that, if the theory of knowledge Critical Idealism presents is not a sound one, the reforming of it will be no mere matter of patchwork, but a rebuilding from the bottom up

But no sense of the cogency of the reasons driving Kant to the doctrine of *a priori* science should blind one to the difficulties facing this philosophy Certain of these must have become so present to the mind that has followed the

argument so far, as to make it reluctant to admit the faults of Empiricism, sensible that the Critical cure threatens to be worse than the Empirical disease The following objections are as obvious as they are serious

The sciences to which *a priori* knowledge is confined are (1) such science as enables us to order our experience in space-time coordinates (geometry), and (2) such science as furnishes us with the concepts by means of which we recognize an object as an object (logic) Since we bring these abilities to experience, we must in some sense bring to experience the sciences which not to possess is to lack such abilities But we all know—and Kant was willing to concede, even to insist on the point—we all know that these sciences are the possession of none but the mature, which is to say, the highly experienced mind We might go even further and maintain that no human mind has yet won a complete insight into the ways either of geometry or of logic The technical journals are filled with patient efforts to put science in more masterful possession of these disciplines One would not be risking much in predicting that, if this cooling planet ever comes to its last day, and if in that day there still appear technical journals, their tables of content will continue to include such titles as, "On the Axioms of Geometry", "On the Postulates of Logic " How then and in what sense can that science which is beyond the grasp of a Euclid or an Aristotle be the possession of a new born babe? Or, to render the matter still more preposterous, does it not seem that a Euclid or an Aristotle must have spent his life in a none too successful struggle to possess himself of a science the possession of which was the condition of his beginning the struggle?

These difficulties in the way of accepting, or at least of resting with, a doctrine of *a priori* science are indeed both

obvious and serious But no sooner has he set them clearly
before him than the student of the history of reflection will
recognize a family resemblance between Kant's troubles
with *a priori* science and old perplexities of much the same
order, which in their day had found solutions no less strange
than his

Nothing could come stranger to modern ears than the
suggestion that what we call learning by experience is
really but a *recollection* of things once known Yet this is the
solution Plato offers to the problem, How may a man know
and not know a thing at the same time? For Plato in his own
way had arrived at a conclusion in which inhered all the
difficulties of later doctrines of *a priori* science He was
much impressed with the fact that we assert many things
to be true that we could not verify by any actual observa-
tion, and hence could not have learned (in the Empiricist's
sense of learning) from previous observations, i e from
experience Thus we are quite confident that if the sides of
a triangle are equal, the angles are so too, but who ever saw
or hopes to see an (absolutely) plane three sided figure
whose sides were (absolutely) equal, and its angles, too?
Wherefore, as the approximate equality we see is unlike the
absolute equality about which we make such confident
assertions, it is plain that experience cannot have taught us
this truth in any ordinary sense of teaching

But there is one instance, familiar to all, in which a
present experience may "call to mind" an idea very unlike
itself, that is when a word, a picture, or the sight of some
object associated with a certain man we have known brings
to our mind his image May it not be then that the part
experience plays in "putting us in mind of" the Ideas of
science, so unlike the things experienced, is just that played
by the object stimulating us to recall a lost memory, also

quite unlike the thing recalling it? But—so Plato's specu-
lations carry him on—in the case of the Ideas of science, we
could no more have met them face to face at some earlier
moment of this life than we can meet them now or look
forward to meeting them tomorrow. They must then have
been slumbering in the soul's possession at its very birth,
memories already lost and forgotten. "Then may we not
say, Simmias, that if, as we are always repeating, there is
an absolute beauty and goodness and essence [of triangu-
larity, for example] in general, and to this, which is now
discovered to be a previous condition of our being, we refer
all our sensations and with this compare them—assuming
this to have a prior existence, then our soul must have had a
prior existence?"[1]

To such lengths have thoughtful men been driven in the
past by the necessity of explaining how we can have pure
science, which, though we apply it to experience and can
formulate only after rich experience, we nevertheless do
not and cannot verify by experience, since experience fur-
nishes us with none of the objects this science is conversant
with.

Or again, it is the most cautious Aristotle who would
explain to his pupil, Theophrastus, how the "active reason",
that organ of pure science, could have been present in the
soul at birth and yet the child remain so unreasoning. And
if Aristotle finds an answer it is still but a figurative one.
This divine organ must gradually attain to the perception of
truth in the light of experience, as the eye of the bat must,
not grow with, but grow accustomed to, the vision of the
day.[2]

No more in the Seventeenth Century of our era than in
the Fourth before it is the problem of the part played by
experience in the generation of pure science (now called

"clear and distinct ideas") overlooked—and no more is it
solved The Rationalist of this time is quite accustomed to
speak of such ideas as "innate", as "imprinted on the soul
at birth", and at times speaks in such serious tones that it is
not surprising Locke should have thought he was preparing
the way of Empiricism by cleansing our "white-paper"
minds of such innate ideas How could Locke, or any one,
suppose the *innate* ideas of Descartes to be anything but
congenital, when we find Descartes himself writing to his
friend Mersenne such "clarifications" of his meaning as
the following

> "Ne craignez point, je vous prie, d'assurer et de pub-
> lier partout, que c'est Dieu qui a établi ces lois [les
> vérités mathématiques] en la nature, ainsi qu'un Roi
> établit les lois en son Royaume Or, il n'y en a aucune
> en particulier que nous ne puissions comprendre si
> notre esprit se porte a la considerer, et elles sont toutes
> *mentibus nostris ingenitae*, ainsi qu'un Roi imprimerait
> ses lois dans le coeur de tous ses sujets, s'il en avait
> aussi bien le pouvoir "[3]

It is only when one has come to see how far from clear
and clarifying, how hopelessly *confused* and *confusing*, are
such "helpful" illustrations as the one offered by Descartes
to Mersenne, that one can no longer look back upon the
arguments directed by Locke against the doctrine of *innate
ideas* as blows aimed at shadows [4] They are all based on the
understanding that the *innate* can have no other meaning
than the *congenital* Among the best considered of these
arguments, is one that practically repeats the questioning
thought with which our last section closed Among the
"clear and distinct ideas", Locke supposes, the Rationalist

would surely include the truths of logic and geometry,—
how then do "children and idiots" not possess these ideas?
How can they not have what is theirs by right of birth? Or
again, if by birth we do not mean physical birth, but some-
thing more like birth of the understanding, must not the
most complicated theorems of geometry be as truly innate
ideas as its simplest axioms, since they are accepted as soon
as understood?

Now, if the Rationalist, in his more reflective moments,
had continued in the thought of his hasty efforts at illustra-
tion, such arguments as the foregoing would be perfectly
effective in reducing the doctrine of innate ideas to an
absurdity—an absurdity into which it is hardly to be
imagined how such minds as those of a Descartes, a Spinoza,
a Leibnitz could have fallen. But it was only in his more
careless moments that Descartes failed to distinguish be-
tween the innate and the congenital. In his more thought-
ful moments we may imagine Descartes returning to
Locke's last question some such answer as this: "Of course
the Pythagorean Theorem must be as truly innate as the
axiom, things equal to the same thing are equal to each
other. To demonstrate whatever theorem, is to prove it
innate."

It follows, of course, that not only are children and idiots
aware of none of the ideas said to be innate in them, but the
wisest of men can never become aware of more than a very
few of the innate ideas which, had they been congenital,
would have been common to all men born into the world.
To explain how man can have a wealth of innate ideas of
which at birth he knows none, and at death can have learned
but few, Descartes in his turn falls back on analogies and
figures of speech; these ideas, he explains, are "[Innatae]
eodem sensu quo dicimus generositatem esse quibusdam

familiis innatam, aliis vero quosdam morbos, ut podagram vel calculum, non quod ideo instarum familiarum infantes morbis istis in utero matri laborent, sed quod nascantur cum quadam dispositione sive facultate a illos contra-hendos "[5]

Evidently then, the Seventeenth Century Rationalist, while perfectly alive to the paradox his teaching implied, is no less driven than his classic forerunners to explain the apparent contradiction implied in an assumption of *a priori* knowledge by invoking some double sense in which knowledge may be said to be "possessed" It is easy to think of many distinctions sanctioned by use, between a greater and less perfection in our knowing of such things as we may be said to know at all, none more common than the difference between knowing "as in a glass, darkly" and know-ing "face to face" On this or on some such distinction the Rationalist is content to fall back, but it is clear we have once more been put off with a figure and a metaphor The Rationalist has given his own name to a problem and called the problem solved

Now while Kant's *a priori* science differs in one notable respect from the "rational science" of the older and of the newer Rationalism, it nevertheless shares with both the fundamental difficulty of explaining how we can at the same time have knowledge and not have it The difference, important enough in other connections, does nothing to relieve this particular situation Kant indeed was strong to deny what Leibnitz had come to maintain, namely, that *a priori* knowledge was definitional in character, for Kant an axiom of geometry, say, had ceased to be an "analytic" and had become a "synthetic judgment" [6] But it was none the less *a priori* for that, indeed Kant's insistence on the impossibility of beginning to gather empirical knowledge were

we not already possessed of *a priori* knowledge brings the
nature of our difficulty all the more vividly before the mind
He is no more blind than his predecessors to the appearance
of paradox, he is no more successful than they in removing it
Nor is this surprising, for the solution, if we have correctly
imagined it, lies outside the range of Kant's habitual
methods of thought, though familiar enough in principle
to those Post-Kantians who, from Fichte on, paid their fore-
runner the tribute of thinking beyond him In a tentative
preliminary way we may say that the solution we seek turns
on the distinction between *what a mind is for itself and
what it is for another*

What a mind is for itself and what it is for another? In
one sense this distinction is so familiar to everyone, so com-
monly made in conversation, that whatever it has to offer
in solution of world-old perplexities must, one would think,
have been at everyone's disposal from the beginning of things
Such, however, has not been the history of reflection on the
mind's possessions and prepossessions We are ready enough,
when it suits us, to claim for ourselves an insight into what
our neighbor "has in mind", truer than any the man pre-
tends to have of his own thoughts, feelings, and motives Yet
we are none too ready to generalize, and to accept the
implications of our own thesis Can it be that the truth
about my mind lies in another's keeping? Can it be that
only another knows what I really am and what I really
think?

The suggestion has its threatening implication, certainly,
and is not to be taken lightly One recalls the annoyance of
the Autocrat when "the young man named John", having
just learned that he was not only himself as he took himself
to be, but also himself as he seemed to his neighbor, and
thirdly himself as his Maker knew him, promptly appro-

priated the three peaches remaining in the basket, with the
remark that there was just one apiece for him "I convinced
him [says the Autocrat] that his inference was hasty and
illogical, but in the meantime he had eaten the peaches"

   We shall do what we can to forestall "hasty and illogical
inferences" that might spoil for us a suggestion not without
promise for our future ease

   [1]*Phaedo*, 76
   [2]*Metaphysics*, B 1 993 b9, *De anima*, III, Vol 2
   [3]Descartes to Mersennes, 15 Avril 1630, *Oeuvres*, ed Adam and
Tannery, I, 145
   [4]*Essay*, Book 1
   [5]*Notae in Programma*, ed Adamnd and Tannery, VIII, 358 B a
   [6]"Analytic," for Kant, was any judgment whose predicate was part of
the definition of the subject, "synthetic", one whose predicate *added*
to the attributes belonging to the subject by definition Thus "A
triangle is three sided' would be an analytic judgment "A triangle is a
figure whose interior angles taken together equal two right angles"
would be synthetic *Cf K d r V*, 10–17 B

6. The *Esse* of Minds *Percipi*: The Reflective Standpoint

WE MAKE LITTLE DIFFICULTY IN ADMITTING THAT WHAT prepossessions a man brings to the judgment of his experience are none the less his for his being unaware of them. In general it may be said, the mind by which prejudices are recognized is quite other than the mind revealing them. This "other" may of course be the prejudiced individual himself in some more reflective or, as we say, "introspective" mood, but the point is that it need not be. And even when it is, the man who reflects is of "another mind" about himself than the mind in which he lived through the experience reflected on. We shall simply speak of this *reflective standpoint* from which an actively experiencing mind is viewed, as *another mind*, without prejudice as to whether it is or is not another man's mind.

However simple the experiencing mind, the reflective mind may be as learned as you please, whether taught by experience or come by its wisdom in whatever way, our general assumption is that the greater its knowledge, the better its fitness to judge any given mentality. Our Autocrat identified John as he appeared to his Maker with John as he "really was", wherein we may catch an echo of the popular sentiment that would have each of us await the Day of Judgment (the judgment of an Allwise Mind) for the only certainty he can ever have as to the kind of man he really is. But for the present we mean to draw on the support of this widespread theology for no more than enough to show that we have no sacred scruples against allowing the truth about a given mind to lie safe in the keeping of another.

It is likely the bearing of these homely observations on the problem we have been discussing will have been anticipated by all If indeed those prepossessions which the least analytic of us recognizes as playing so large a part in moulding his neighbor's (and who knows but his own) judgment of things are plainer, more evident, more demonstrable to the reflective other-mind than to the judging mind itself, why should not this principle guide us to an understanding of the "limiting case" the case of the new-born learner whose only possessions have to be prepossessions?

With a little good-will the reader's ingenuity may suggest to him plausible and quite familiar candidates for the role of the infant's *a priori* equipment, such an equipment as the newcomer at once uses, yet never knows he possesses We have all remarked how prompt this newcomer is to do many things he could never have learned the art of *in utero* Very efficiently when given the nipple does the babe begin to suck, and we readily see that but for this happy "provision of Nature" there would have been no mammalian race to hand on its congenital equipment from one generation to another However this advantage was come by, whether as a result of accidental variations or as an accumulation of inherited acquirements, we are content to attribute such early "possessions" of life to the fact that life has gone before In some sense then the experience of the race furnishes an equipment to the individual at the very outset of his career, an equipment which, if it is used by the learner himself, is of course only evident to and understood by the onlooker, the reflective mind

So completely does this legacy taken in all its wealth seem to fulfil the requirements of Kant's *a priori* equipment that many excellent minds have considered Kant's sole contribution to have lain in calling attention (in a curious

round-about way, to be sure) to the part played by inherit-
ance in the process we call learning-by-experience We have
only to add "race-experience" to individual experience to
let the whole gamut of learning be run within the range of
experience This congenital equipment, which makes the
new-born mind so much more than a blank sheet of "white
paper", has sometimes been called the mind's "physio-
logical" and sometimes its "empirical" *a priori* As one
writer sees the situation

> "The protest of Rationalism and Apriorism against
> the sensualistic account of all experience is justified,
> but the actual development of this protest in [Kantian]
> Apriorism was perverse and wrong To correct sensual-
> ism it would have been quite enough to assume a
> physiological a priori When we propose the idea of a
> physiological a priori we assume in the brain of man at
> birth a certain preparedness for reacting in set ways
> to the impressions of the outer world Just as the new-
> born child feels within itself a nutritive instinct with-
> out ever having found by experiment that food stills
> hunger, just as it applies itself to the breast without
> being able actually to know that there it will find the
> needed milk—just so are there intellectual instincts
> the physical acts of comparing, combining, abstracting
> may be nascent in the human mind before it has had
> any experience whatever For very little we might
> venture to put a thought (Gedanke) back of the child's
> turning to the mother's breast This way of acting is
> equivalent to the thoughts that there is nourishment to
> be had Many of the deepest and most intelligent
> thoughts that come to us in later life define themselves
> no more clearly in their first germination If it be but

admitted that our conscious thinking may be tracked back to this border of the unconscious, that beyond this in some last faint glimmer of consciousness a thought may be divined, then perhaps we might venture to say in that first instinctive stirring of the child lies misty and as yet latent an a priori thought   But not to give unnecessary offense to those who ascribe to pure mentality a meaning independent of physical reference, it were better to avoid the word physiological Let us say then there is a small but all essential store of experience handed on at birth from parent to child We may leave it to each to think as he will how this transfer is effected—whether its manner is physical or psychical "[1]

But no, a little reflection will show that however heavily the individual may be indebted to those who have gone before for ability to take his first steps in life, it is evident that whatever force there is in Kant's criticism tells against the beginning of *experience*, quite as much, then, against the beginning of race acquisition as against the start off of individual learning by experience If Kant's reasoning holds for any, it must hold for all examples of learning by experience, each must be equipped with all that "makes experience possible " What Kant takes this equipment to be, we have already seen the coordinates of (space time) geometry, the categories of logic This equipment is as essential to the first steps of the experiencing amoeba (if from some such ancestor we are sprung) as it is to the incipient learning of a Euclid or an Aristotle Which is absurd, if you please—or else it may be vastly enlightening

Put, if you will, the contention of Criticism in this way Wherever there is experience there are prerequisites of

experience. So far we have been content to follow the traditional interpretation of this insight, an interpretation common to the "logical" and the "psychological" theories of the *a priori*, which takes it for granted that the experience and the prerequisites of this experience *belong to the same subject*. From this assumption all our difficulties have sprung. What we now propose is the abandonment of the classic interpretation in favor of this new one: Experience does indeed presuppose possession, but *the experience belongs to one subject* (the learner) *the possessions to another* (the reflective onlooker). So that we may say, the real contribution of Criticism is to have shown that *every experiencing mind presupposes a reflective mind.* Which statement, if it sound strange to the ear, is after all no more than the sentiment of our Autocrat: John *is*, in each moment of his inmost life, what he appears to Omniscience to be. The *esse* of his mind is *percipi*.

In the statement that an experiencer exists only as he exists for another, what most offends the ear of common sense is naturally enough the feeling we all share, that what we live through we live through unaffected by the accident of our being overseen and thought over by another, or recalled and reflected on by ourselves. Suppose we were unobserved or that we never happened to recall and reflect upon this moment so immediately and fleetingly lived, would that in any way change the content of the moment? Could it leave it in any way poorer than it would have been had a hidden eye been spying upon it, or a future mood brought it back for our own riper consideration? These are the questions that have always been directed against those who would maintain the "being" of mind to lie in its "being perceived"; i.e. in its contents being known, not immediately, but only on reflection.

These observations are so sound, so consonant with all we know about facts and our perception of them, that if they constitute any objection to the point of view we have here taken it would be an absolutely fatal one. That they should be urged at all, however, shows nothing more clearly than that we have not succeeded in making the meaning of our position clear.

Let us admit that no amount of reflection on a fact can change that fact. The real meaning of this admission is best put in the converse form: a fact is that to possess which is to possess what no reflection can alter. But who, we ask, possesses such a fact, when the facts of experience are in question? Is it the one who is said to experience it? If by possessing the fact we mean being the person the facts of whose experience are in question, of course they are his facts and not his neighbor's. But if by possessing these facts we mean being in position to name, recount and appraise them, then it is an illusion as common as it is dangerous to suppose that the one whom events befall is in the best case to tell what these events are. It is not to him we should turn—certainly not to him alone—if we would get at the facts of the case. Lay writers have seen no difficulty in recognizing either the principle or its importance.

"Look into your own heart and write! said Herr Kant; and earth's cuckoos answered the cry. Look into the Rhine where it is deepest, and the Thames where it is thickest, and paint the bottom. Lower a bucket into a well of self-deception, and what comes up must be immortal truth, mustn't it? Now, in the first place, no son of Adam ever reads his own heart at all, except by the habit acquired, and the light gained, from years' perusal of other hearts; and even then, with his

acquired sagacity and reflected light, he can but spell
and decipher his own heart, not read it fluently "[2]

To have laid hold on the facts of experience in the sense
of being able to tell what they are, is the function of the
reflective mind—a function that can be performed by no
other The *fact* meanwhile is, or would be if ever grasped,
such a possession of the reflective mind as no further
experience and reflection could alter the complexion of It
is toward this conception of the fact that our whole debate
up to this point has driven us forward, it is with facts and
laws as they exist for reflection that we shall be occupied
throughout the sequel It may well be that the sense in
which these facts are "hard" and their laws "inexorable"
will appear in this sequel in an entirely new light and that
the interest of the adventure among them will repay us for
what there has been of the tedious and painstaking in our
preparation for it

[1]Walter Frost, *Naturphilosophie*, I, pp 199–202, selected A similar
"physiological" or 'empirical' *a priori* is proposed by Rehmke in his
discussion of space perceptions

"Apriorische Ursprunglichkeit des Raumbewusstseins will sagen,
dass die Seele, wann immer sie da ist und Empfindungen hat, zugleich,
aber kraft einer in ihr Bewusstsein liegenden Eigenart allein, Raum
bewusstsein hat, also ohne eine seiner Bedingungen in vorausgehenden
Vorgangen des Nervensystems habe Empirische Ursprunglichkeit des
Raumbewusstseins dagegen behauptet eine physiologische Bedingung
fur dasselbe   fur jene ist das menschliche Bewusstsein der
Schopfer", fur diese aber ist es nur die eine Bedingung des ursprung
lichen Raumbewusstseins

' Wir finden keine Veranlassung, zu einer apriori Theorie des
Raumbewusstseins unsere Zuflucht zu nehmen und von unsere
Meinung der empirischen Ursprunglichkeit des Raumbewusstseins
abzulassen" *Lehrbuch der Allgemeinen Psychologie*, pp 209–211,
selected

¹Charles Reade, *The Cloister and the Hearth*, 1, 6

So, too, the illusory character of "immediate and certain self knowledge", seems frequently to have impressed itself on the mind of Lawrence Sterne Two of his sermons have this "lesson" as their central theme, the one was later introduced into *Tristram Shandy* as a homily or Yorick, the other, delivered on the text, "Thou art the man   " 2 Samuel, XII, 7, is worth recalling

"There is no historical passage in Scripture, which gives a more remarkable instance of the deceitfulness of the heart of man to itself than this   To know one's self, one would think could be no very difficult lesson,—for who, you will say, can well be truly ignorant of himself and the disposition of his own heart? If a man thinks at all, he cannot be a stranger to what passes there—he must be conscious of his own thoughts and desires, he must remember his past pursuits, and the true springs and motives which have directed the actions of his life he may hand out false colors to the world, but how can a man deceive him self? That a man can is evident, because he daily does so —Scripture tells us, and gives us many historical proofs of it, besides this to which the text refers—'that the heart of man is treacherous to itself and *deceitful above all things*,' and experience and every reverie of his own commerce with the world confirms the truth of this seeming paradox, 'That though man is the only creature endowed with reflection, and consequently qualified to know the most of himself—yet so it happens, that he generally knows the least—and with all the power which God has given him of turning his eyes inward upon himself, and taking notice of the chain of his own thoughts and desires—yet   he is as much, nay often a much greater stranger to his own disposition and true character, than all the world besides "

7. Retrospect and Prospect

BEFORE TURNING TO THE FUTURE, A FINAL GLANCE AT the past One would not care to leave the limited but important period of history, to which this view has confined itself, without coming to some sharper decision than has yet been thought out on a question that our proposed solution of the "paradox of the *a priori*" makes critical This solution depends for its enlightenment on a distinction drawn between "what is known to experience" and "what is known to reflection on experience " How remote is this distinction from anything Kant himself might have accepted as a solution of the paradox, or, what comes to the same thing, as a reason why, to his understanding, there was no paradox calling for solution? This is a question on which interpreters of Kant would differ so widely that to discuss it with any faithfulness to Kant's own wordings would require a documentation beyond the reach of an introduction intended to be brief But if, to avoid letting ourselves exaggerate the novelty of our solution, we look for that interpretation of the old which would minimize the newness of the new, a few passages of the first *Kritik* may suggest that the distinction on which this solution rests is none other than one drawn by Kant himself It is a distinction on which depends the very meaning of his "transcendental philosophy " This is not to say that after a century and a half of the most varied practice in reflecting on an ever-widening range of experience, men are likely, in the manner, matter, or conclusion of their reflections, to come out with results similar to, or even

consistent with, Kant's But it is to say that in the modern scientist's willingness to recognize in every experimental outgiving an adjustment, generally unconscious, of found matter to made forms, science is letting itself take a reflective standpoint that brings the method of scientific thinking closer to Kant's transcendental philosophy than to either the Rationalism or the Empiricism that had gone before

Although *reflection*, its derivatives and combinations, is a concept of which Kant makes important use in the discussion of special problems,[1] it was never for him the key-word that it came to be for certain Post Kantian idealists[2] For that role, he made the unfortunate choice of the adjective *transcendental*, with the result that, as he himself came belatedly to foresee, the *transcendental* was bound to be generally taken to refer to what transcends all possible experience Such an interpretation would make the transcendental not only impossible of perception *in* experience, but impossible of conception *in terms of* experience Realizing this danger too late to avoid it, Kant tries to warn against it by drawing an entirely artificial distinction between the *transcendental* and the *transcendent*,[3] a distinction that he has not himself offered in the course of working out his thought But if, in observance of the wish expressed in this passage, we transfer to the *transcendent* the meaning just "mistakenly" accorded the *transcendental*, what remains for the transcendental to "transcend"? Would it not seem that what is "transcended," without including *all* experience, could meaningfully enough refer to an *immediate* or relatively immediate experience? In which case the transcendental view-point would begin to take on the meaning of the *reflective*, and the mind studying its immediate experience from the transcendental point of view would become, what Fichte later calls, *das reflectierende Bewusstsein*

How far the text supports this interpretation of Kant's
meaning must be left to the judgment of each reader, hav-
ing before him the more significant passages of the
K d r V, of which the most important is reproduced in the
subjoined footnote ⁴ Of course, this interpretation does not
relieve one of the difficulty he may experience in under-
standing how a reflective view-point could be more than
relative, how it could discover and establish such eternal
truths as Kant took his "system of all axioms of the pure
understanding" to be ⁵ Some, indeed, would take this short-
coming of the Critical philosophy to be the defect that
makes certain developments of the Post-Kantian period a
needed and illuminating sequel to the Kantian contribution,
a contribution which the idealists of this period were ready
enough to recognize and value, but not to rest content with
It is certain that the initial and long enduring hostility of
experimental science to Kant's system rested on the experi-
mentalist's understandable distrust of *a priori* truths If
certain developments of quite recent times have brought
about a modification of this attitude, it is because they have
made it possible to distinguish a doctrine of *a priori forms*
from a theory of *a priori truths* This distinction made, it is
possible to recognize the importance to later science of Kant's
reflections on the part played by the formal sciences in
designing patterns, no longer taken to be unchangeable, to
which empirical data must be "adjusted," before experi-
mental science can formulate its findings When, in the
sequel, the significance of this step in the development of
experimental method will have been more fully examined,
it may be that one will feel the experimental scientist of
today to be much closer to his old enemy, Kant, than to his
former ally, the Eighteenth Century Empiricist But that is
for the sequel to consider Meanwhile, something may yet

be done to make the results of this brief review of the past
more available than our "dialectic" has yet made it to the
constructive thinking of the future

One who finds himself dissatisfied with the thought of the
past on whatever subject will do well, before developing his
own reflections, to construct if he can a frame of classifica-
tion that shall accommodate all historic theories in compart-
ments designed to meet the demands of formal logic Only
so can he discover possibilities of new development that
shall be sharply differentiated from all historic theories as
each of these is differentiated from each other

The demands of formal logic general to all schemes of
classification, whatever the "things" to be classified, are two,
namely, that the classes be (1) mutually exclusive, and (2)
together exhaustive of all the things to be classified That is,
everything must find a place in one and only one class
When the things to be classified are opinions, we may con-
sider each opinion to be equivalent to a set of propositions
To construct a set of propositions strictly equivalent to an
historic theory of evidence would be to define that theory in
terms of a "postulate set," meeting all the requirements
formal logic imposes on such a set ⁶ This would be a difficult
and lengthy undertaking, entirely beyond the reach of a
brief historical introduction But, for purposes of classifica-
tion intended to do no more than establish a difference of
doctrine between schools to which the preceding chapters
have attached different names, a less elaborately conditioned
proposition-set will serve It will be sufficient to compare
our schools in terms of suitably chosen proposition-sets, each
set *implied* by, but *not* necessarily *implying* (and there-
fore, not pretending to define) the opinion of one of our
three theories of evidence But even to do this much is a
critical undertaking It is most critical, but also uniquely

satisfactory, when the mutual exclusiveness and collective exhaustiveness of the classes to which differently-named opinions are ascribed is to be established on purely *formal* evidence. For the difference, the incompatibility of two opinions is formally established, if and only if each is shown to imply a proposition that is the formal contradictory of a proposition implied by the other. But the contradictory relation of two propositions is formal, if and only if, it is independent of the meaning of the terms in which the propositions are expressed (As, in formal logic would be, *All a is b* and *Some a is not b*, or, P_1 *implies* P_2 and P_1 *does not imply* P_1) The collective exhaustiveness of the postulate sets differentiating of the opinions to be classified is no less to be tested by such complications of formal logic as will be observed in the sequel. But how subject the verbally expressed differences of opinion that have served our review to differentiate its schools to a test of incompatibility so rigorous? As this is the sort of question that must face any careful thinker wherever he is called upon to classify opinions on whatever subject, it will repay us to examine with some care this particular case of a very general and important problem

Any proposition, of which every opinion of a given array implies either the affirmation (P) or the denial (P'), will divide the array into two classes of formally incompatible opinions those implying P and those implying P' Any two propositions (P_1, P_2), of which neither implies the other (i e they are mutually independent) nor the contradictory of the other (i e they are consistent), and such that every opinion of an array implies either the affirmation or the negation of each, will furnish a formal classification-frame of four compartments, $P_1 P_2$, $P_1 P_2'$, $P_1' P_2$, $P_1' P_2'$ In such a scheme of classification, the mutual exclusiveness of any

two classes would be formally established, since each class would affirm at least one proposition denied by each other The exhaustiveness of the scheme is no less formally evident, there can be only four consistent and mutually independent combinations of the elements, P_1, P_1', P_2, P_2' taken two at a time Since any two propositions fulfilling the conditions laid on P_1 and P_2 provide a frame of four compartments, and since we have to consider only three historic schools of opinion, no more elaborate frame is needed for their classification [7]

One readily sees that classification of materials in formal frames is the only one that demonstrably satisfies the logical requirements of mutual exclusiveness and collective exhaustiveness imposed on all schemes of classification For only in this way can the "verbal incompatibility" of opinions be made to depend for its justification on a formal incompatibility, i e on an implied contradiction for which the evidence is independent of the meaning of the terms in which the original historically worded opinions happen to have been expressed One can well imagine how important is this independence of word-meanings, when the things to be classified are opinions whose original expressions reflect the cultural setting of many different periods of a long past Your traditional historian of opinion is seldom if ever at pains to show his differently-named schools of thought to deserve their difference of name, by showing each to imply a proposition that is the formal contradictory of a proposition implied by the other The results are anything but convincing If, for example, this historian tells me that *realism* and *idealism* are opposing schools of opinion, I can only answer him, "Perhaps, as you understand the terms, but your understanding of realism and idealism has not been so widely shared as to prevent more than one

satisfactory, when the mutual exclusiveness and collective exhaustiveness of the classes to which differently-named opinions are ascribed is to be established on purely *formal* evidence For the difference, the incompatibility of two opinions is formally established, if and only if each is shown to imply a proposition that is the formal contradictory of a proposition implied by the other But the contradictory relation of two propositions is formal, if and only if, it is independent of the meaning of the terms in which the propositions are expressed (As, in formal logic would be, *All a is b* and *Some a is not b*, or, P_1 implies P_2 and P_1 does *not imply* P_1) The collective exhaustiveness of the postulate sets differentiating of the opinions to be classified is no less to be tested by such complications of formal logic as will be observed in the sequel But how subject the verbally expressed differences of opinion that have served our review to differentiate its schools to a test of incompatibility so rigorous? As this is the sort of question that must face any careful thinker wherever he is called upon to classify opinions on whatever subject, it will repay us to examine with some care this particular case of a very general and important problem

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important thinker (after the manner of Fichte) from denominating himself a *realistic idealist*, or an *idealistic realist*. Only if you can show me that, of the *realism* and *idealism* you have in mind, the one implies a proposition, P, the other, its contradictory, P′, can I accept your classification as having any logical significance."

Unfortunately, one cannot command the advantages of formal classification without submitting to its exactions, and a demand on which depends the very possibility of such classification is notoriously difficult to meet. Thus, to effect a formal classification of our schools, we must find two propositions fulfilling the conditions laid on P_1 and P_2 But where look for two such propositions? If indeed all philosophers had shared and successfully realized the ambition of Spinoza, to present a theory of evidence "after the manner of geometry," the formal classification of philosophies would be no more of a problem than is the like classification of geometries And this presents no serious difficulties Suppose, for example, one wanted to construct a classification-frame to accommodate just the three most familiar types of historic geometry, Riemannian (*R*), Euclidean (*E*), Lobatchewskian (*L*). Each has been presented in the form of such a highly perfected "deductive system" as to permit us at once to find among their respective postulates two propositions, P_1, P_2, of which *E* accepts $P_1 P_2$, *R*, $P_1 P_2'$, *L*, $P_1' P_2$, while the remaining compartment $P_1' P_2'$, remains "historically empty."[8] No such triad of perfected deductive systems awaits the classifier of philosophies Yet this very example of the geometer's unshared ease, while it lets us realize the difficulty of a formal classification of philosophies, happens to carry with it a suggestion that may help us in the end to overcome this difficulty.

One recalls that a formal classification of these three

geometries can be perfectly well effected without referring to the first premises, the postulate-sets, on which they severally rest. Among the theorems of each system, is one that relates the sum of the interior angles of a triangle to two right angles. For R, this sum is greater than; for E, equal to; for L less than two right angles. Could we not, one asks, find some two propositions, consistent and mutually independent, such that, of our three theories of evidence, each implies an attitude of acceptance or rejection of each of these propositions, and no two theories imply the same attitude?

In the search for two such propositions, one is not left so entirely to one's own ingenuity and contrivance as might at first be supposed. There are considerations springing from the problems general to all theories of evidence that serve to guide us. Thus, we reflect that most, if not all of the questions to which practical humanity seeks answers are to be classified under questions of (individual) fact, or questions of (universal) law. No theory of evidence, then, could have failed to consider the kind of evidence on which we should base our *response* to questions of either kind, where by *response* is meant a proposition that purports to convey one's "knowledge of fact" or "knowledge of law." Then, we could have anticipated, without waiting for the testimony of history to verify the surmise, that the issue of dependence or independence of either kind of knowledge on the other must be central for each theory of evidence. We have found it indeed to be not only central, but critical to a degree that suggests at once candidates for the roles of those two mutually independent and consistent propositions, P_1, P_2, required for the construction of a formal classification frame. They are the proposition, "Knowledge of fact implies knowledge of law," and its converse, "Knowledge of law implies

knowledge of fact " It remains to be seen, first, how far the summary account the preceding *Dialectic* offers of the three schools it has reviewed, will enable us to establish the "attitude" of each of the schools toward each of the propositions and, second, whether from such attitudes as we are able to establish, we may construct a classification-frame that will assure the mutual exclusiveness of the classes to which these schools are to be severally assigned Finally, since our three schools can fill but three compartments of a four compartment frame, we must consider whether there is any known reason why the compartment that our three historic schools leave historically empty must remain as much of a void to later centuries as it did to the closing Eighteenth

The propositions our three schools are here taken to imply are, respectively,

(1) *Rationalist* —No knowledge of law implies knowledge of fact, all knowledge of fact implies knowledge of law

(2) *Empiricist* —All knowledge of law implies knowledge of fact, some knowledge of fact does not imply knowledge of law

(3) *Critical* —Some knowledge of law implies, and some does not imply knowledge of fact, some knowledge of fact implies and some does not imply knowledge of law

(1) That the Rationalist theory of evidence implies the first clause of the proposition attributed to it, is most readily shown by recalling the Leibnitzian demand, that all science establishing universally valid laws should be based on no other premises than definitions, "which as they depend only on ourselves, have no need of eternal support " In this, Leibnitz does not depart from the meaning of Descartes and Spinoza, but merely clarifies their common meaning and makes it explicit

That the Rationalist theory implies the second clause of the proposition attributed to it, will be evident when we recall that, to keep our knowledge of the factual world independent of sensuous evidence, it was necessary to suppose knowledge of this world to be deducible from the goodness of that *ens perfectissimum* which was its cause, while both the existence and the goodness of this *ens* were themselves deducible from its definition It may be that in this explicit statement of the way in which knowledge of individual fact, to be kept independent of sense observation, must be made to depend on knowledge of universal law, Leibniz has said more than either of his predecessors put into words, but if so, it is because neither Descartes nor Spinoza said enough on the subject to make it clear *how* a Rationalist was to save knowledge of fact from dependence on sense Yet, if Rationalism was to survive as a theory of evidence, it had to find some way of freeing knowledge from all dependence on sense observation

(2) That the Empiricist theory implies the first part of the proposition attributed to it, hardly needs argument, one has only to recall Locke's statement concerning the two sources of all our knowledge, simple ideas of sense and simple ideas of reflection Since all knowledge is made to depend on knowledge of these simple ideas, Empiricism could admit no knowledge of law that did not imply knowledge of such facts as these

That Empirical theory implies the second of the doctrines attributed to it, is shown in the sequel to the passage cited This sequel, in dividing all ideas into the simple and the complex, recognizes that our knowledge of the simple implies no knowledge of law whatever In its further development, however, Empiricism recognizes knowledge of some at least of our complex ideas to imply knowledge of

law, *the laws of inference*. But knowledge of these laws, indispensable to our knowledge of facts not immediately given, while not itself dependent on immediately known facts alone is none-the-less dependent on previous knowledge of facts immediately given. How knowledge of the laws of inference are come by, is elaborately studied by the later Empiricists (*e.g.* Mill) under the head of *inductive logic*. However unconvincing may be the attempt to make inductive logic a purely empirical science, the attempt to do so shows that, to the mind of the consistent Empiricist, our possession and use of laws of inference in coming by a knowledge of facts not immediately given, does not contravene our statement of the first doctrine of Empirical theory. *All* knowledge of law does, to the Empiricist, imply knowledge of fact. (3) Few words are needed to justify our statement of the critical doctrine attributed to Kant. The laws to know which implies no knowledge of fact are the *a priori*; those to know which presupposes knowledge of fact, the *a posteriori*.

The facts to know which implies no knowledge of law, are the immediate data of experience, *die reine Empfindungen*; those to know which does require inference based on acceptance of law, are (with the Critic as with the Empiricist) all facts except these immediate data.

The formal classification-frame to be constructed on the basis of these distinguishing points of doctrine can be made very compact by borrowing from the uses of modern logic some of its more elementary symbols.[9]

Thus, let

L	represent	*knowledge of law*
F	„	*knowledge of fact*
$(X \angle Y)$	„	*all X implies Y*
$(X \angle Y)'$	„	*some X does not imply Y*

then, our frame will take the form

	$L \angle F$	$(L \angle F)'$
$F \angle L$		Rationalism
$(F \angle L)'$	Empiricism	Criticism

A glance at this frame shows at once the formal incompatibility of the three great modern schools towards which, and toward which alone this brief historical introduction planned to orient our thought toward the past before directing it toward the future This *Dialectic of the Schools* would have been of interest to none but the historian did it not suggest to one who has followed its argument, that so much as it has reviewed is badly in need of a future, if a satisfactory theory of evidence is ever to be developed And this brings us to consider that compartment of our four-compartment frame that remains empty

After the remaining compartments have accommodated the three historic schools of Rationalism, Empiricism, Criticism, is there no theory of evidence to be found in history, that holds knowledge of law to depend on knowledge of fact *and conversely*? It is a question reserved for the final classification of theories with which a study of the sort we are about to enter on may properly be expected to close It may be that we shall then find our own thought to have found its place in the compartment now left empty If so, it will be important to consider whether or not any other theory to be found in pages of history not yet examined shares this compartment with our own If any do, it will remain to be seen whether the theory to be presented in the sequel is sufficiently differentiated from such others as share

with it the generic likeness of "being in the same box" to
constitute a new species of the genus If not, our study will
end in an acceptance of some known theory of evidence
What is certain at the outset is that no theory of this genus
could possibly be classed as a form of Rationalism, Empiri-
cism, or Criticism

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[1] E g , K d U , sec 75 *passim*

[2] In Fichte and Hegel, *reflection* is a term whose meaning one must
gather from watching the process in constant operation rather than
from depending on this or that concise statement which might be taken
as a definition of the concept Of this use, perhaps as rich an example as
any is to be found in Fichte is the discussion appearing in his *Grundlage
der gesamten Wissenschaftslehre*, 2ter T sec 4 If one would have a
single sentence that puts in fewest words Hegel's all pervasive thought,
that all we can meaningfully say is said on reflection, perhaps the follow
ing would serve as well as any ' Die einfache Unmittelbarkeit ist selbst
ein Reflexionsausdruck, und bezieht sich auf den Unterschied von dem
Vermittelten   *Logik*, I, "*Womit muss der Anfang der Wissenschaft
gemacht werden* ' (*Werke*, Berlin 1841, III, 58)

[3] K d r V , 352/3 B

[4] Und hier mache ich eine Anmerkung, die ihren Einfluss auf alle
nachfolgende Betrachtungen erstreckt, und die man wohl vor Augen
haben muss, namlich dass nicht eine jede Erkenntniss *a priori*, sondern
nur die, dadurch wir erkennen, dass und wie gewisse Vorstellungen
(Anschauungen oder Begriffe) lediglich *a priori* angewandt werden,
oder moglich sein, transszendental (d i die Moglichkeit der Erkenntnis
oder der Gebrauch derselben *a priori*) heissen musse Daher ist weder
der Raum noch irgend eine geometrische Bestimmung desselben *a
priori* eine transszendentale Vorstellung sondern nur die Erkenntniss,
dass diese Vorstellungen gar nicht empirischen Ursprungs sein, und
die Moglichkeit, wie sie sich gleichwohl *a priori* auf Gegenstande
der Erfahrung beziehen konne kann transszendental heissen Im
gleichen wurde der Gebrauch des Raumes von Gegenstanden uber
haupt auch transszendental sein aber ist er lediglich auf Gegenstande
der Sinne eingeschrankt, so heisst er empirisch Der Unterschied des
Transszendentalen und Empirischen gehort also nur zur Kritik der

Erkenntnisse und betrifft nicht die Beziehung derselben auf ihren Gegenstand " A d r V 80/1 B Cf 25B, 6dB, 352/3 B

[4] K d r V 187Bff

[5] Requirements, of consistency, non redundancy, sufficiency

[6] Had there been a richer array of things to classify, a frame of com partments sufficiently numerous to accommodate them all in mutually exclusive classes could have been constructed by increasing the number of propositions fulfilling the conditions laid on P_1, P_2 The number of compartments established by n propositions meeting these conditions is given by the formula 2^n

[7] Where, as in geometry, the propositions of a formal science rest on the postulates not of logic only, but of other sciences superimposed upon logic (here, arithmetic and geometry), classification frames can generally be constructed of fewer compartments than are required for frames built on logic alone

[8] The symbolism followed in this study is that used by H B Smith's *Formal Logic*

Part II

8. Facts of Experimental Science

FOR ONE WHO WOULD CONSTRUCT HIS OWN THEORY OF evidence, there is no better starting-point than one that takes up for examination the doctrine most widely accepted at the moment of beginning At the present moment, it would be hard to find anyone who rejected as utterly false the common saying, "We learn by experience " And it would be almost as hard to find anyone who would object to an illustration commonly offered of a knowledge gained by experience, namely, the example of a "science based on experiment " Finally, of all the sciences generally called "experimental," none is supposed to furnish a better example of the experimental method at work than the science of "experimental physics " At any rate, all other sciences depending on experiment to answer their own special questions are forced to accept the physicist's results as true enough to serve as the presupposition of all further experimenting, so that, if physics be not sound, no other experimental science can be

If we enquire as to the reason why so young a science should have come to inspire so large a faith in so large a public, one answer is bound to come to us sooner or later is this The modern world's confidence in the methods of physics is due to the effectiveness with which these methods exclude the nature of the experimenter from the nature of his results The physicist is supposed to be completely controlled by facts, as an observer, he can only record facts, as an interpreter, he can only search for principles that fit the

facts recorded He himself, his hopes, wishes, preferences are for nothing in the result, the image of reality to which his science guides him reflects nothing of his personality, true for one, it is true for all, it is universally true To a world that remembers long periods in its history when the propositions men were obliged to assent to, were forced upon them by the preferences of influential believers, nothing could be more welcome than a philosophy that transferred the authority to force belief from men to facts It is true, that to certain natures the despotism of feelingless fact is more terrible to contemplate than the tyranny of passionate men, and these temperaments have often rebelled emotionally against the dominion of experimental science But it will generally be felt that no such rebellion can dethrone thoughtful science in favor of feelingful insights, for the propositions such insights would endorse are either such as a fuller knowledge of fact must confirm or refute, or they are not If they are, they can at best be no more than intelligent guesses, if they are not, they become unintelligible gestures

However, our first interest as philosophers is not with the beneficence or maleficence of a theory of evidence purporting to be historic, it is rather with the adequacy of the foregoing account of the experimental scientist's position and of its place in history Assuming the attitude of a reflective mind, we would contemplate the experimenter at work, equipped with such means as we have at hand, we would consider whether this investigator's behavior does in fact imply his acceptance of the theory of evidence attributed to him Does the experimental physicist indeed suppose his observations to put him in possession of facts? Do facts in his possession force his later formulation of laws? Is his final image of reality one from which every trace of choice and art has been eliminated?

A first suspicion that there may be some inaccuracy in the layman's familiar account of the scientist's experimental procedure, is born of an easily noticeable discrepancy between the layman's story and the scientist's practice For the layman, the experimenter first possesses himself of a number of "hard facts," and to these he fits the only law that can be made to fit them The experimental scientist, on the other hand, is careful to set it down that he begins, not with any facts to which laws of physics could be made to fit, but with *observations* that purport to be observations of the facts he is after These observations, each one of them and all of them together, he is careful to distinguish from any fact that could possibly force the acceptance of just one to the exclusion of every conceivable other physical law, so careful, that the layman has only his own inexperience to blame if he assume the experimenter ever to have come in possession of a single fact apt to determine a law of Nature The experimenter himself knows that he always has been, is, and always will be confined to the data of observation, and if there is one thing an "observation" can *not* be, it is the thing said to be "observed " No doubt the two conceptions are somehow wedded in our minds and cannot be divorced in thought, but for that very reason the "observation" can no more be the "fact observed" than a husband can be his own wife, or a wife her own husband, indispensable as each is to the being of the other

It does seem a paradox that an experimenter should be at the same time one who constantly insists on the necessity of controlling theory by fact, and one who just as constantly insists that he neither is nor can be in possession of the sort of fact by which his kind of theory could be relieved of all that is hypothetical A famous attempt to reduce this paradox to something more comprehensible is that Eighteenth

Century philosophy already reviewed under the name of Empiricism Empiricism proposed to correct the impression of the unreflective, that a Galileo (say) begins with such facts as a *length*, or *time-interval*, experimentally determined Galileo, Brahé, Kepler, any other experimenter that ever was or could be, begins with only such facts as together make up his "observation", and as the "observation" can be nothing but the observer's mental state when observing, the only facts given to science are such facts as are properly to be called ideas The simplest of these ideas are, as one will recall from Locke's pages, those that a first experience imprints on the "white paper" of the new-born mind "In [such] experience all our knowledge is founded, and from that it ultimately derives itself Our observation is that which supplies our understandings with all the materials of thinking [Its fountains] are the fountains of knowledge, from whence all the ideas we have or can naturally have, do spring "[1]

The comfort to be derived from this Lockian account of what is meant by "learning by experience" is considerable, so long as one can rest in the assumption that all ideas are facts, and all facts are ideas In particular, calling the experimenter's "observation" an "idea" and the "thing observed" another (more complex) "idea" makes it clear that the experimenter from infancy on has some facts, and to grow indefinitely knowing has only to acquire more He may if he wants, and will if he is wise, let his opinions be utterly controlled by the facts (ideas) that pour in on him, and as no other facts are in question, his whole science may thus be "factually founded " All this makes such smooth-going for one who would have all evidence to be based on the evidence of facts, and all facts to be in the nature of sense-data, that it is no wonder the later Empiricists held

it a matter of reproach to the founder of their school, that he soon abandoned his original plan For, as it turns out, Locke was not long in admitting to the realm of existent things facts of an entirely different order from any of those ideas, simple or complex, to which he first proposed to confine both our observations and the objects observed To this step he felt himself driven by the most cogent of reasons "because [as he explains] not imagining how these simple *ideas* can subsist by themselves, we accustom our selves to suppose some *substratum*, wherein they do subsist, from which they do result, which therefore we call *substances*"

We "accustom ourselves" to suppose the factual existence of these substances, because we cannot do otherwise and still answer our own question What is the *this* which is white to the eye, hard to the touch, sweet to the taste when we perceive *this* piece of sugar as distinct from the *that* of another piece of sugar in which we perceive the same qualities? How can we escape the "supposition" of certain unperceived existences, if we would make our speech intelligible? But how very different are these facts, called *substances*, from those facts, called *ideas*, that seemed at first to control all our knowledge and found all our science? For, unlike any object of knowledge toward which experience might lead the learner, this object called a *substance* is one that "if any one will examine himself concerning his notion of [it], he will find he has no other *idea* of it at all, but only a supposition of he knows not what support of such qualities, which are capable of producing simple ideas in us"[2]

Had the attempt of English Empiricism to resolve the paradox of a philosophy purporting to be that of the experimental scientist ended with Locke, we could only conclude that it had ended in failure In its beginnings, the Empiricist solution of the paradox had seemed most promising

To the popular understanding, experimental science begins with facts; to the experimenter's own understanding, it begins with observations, and can by no possibility pass beyond observation to lay hold on the fact observed. Whereupon, our Empiricist pretends to reconcile all conflict of opinion by pointing out that the experimenter's "observation," his "perception," his "idea," is itself a fact; that this and no other is the fact by which all experimental science is controlled. To be sure, this makes the physicist's fact a mental fact, a fact existing nowhere save in his mind; but at least this fact does exist, and the experimenter must be in possession of it, if he is in possession of his mind. So that if Locke could have let this be his last word on the factual data of knowledge, all would have ended well for his account of a science built solely on experience. But we have just seen that Locke could not let matters rest here; he could not get over the impression that an idea was a something whose existence in a mind depended on the existence of a something outside of that mind to which the idea was related and "from which it did result." But what that something was, how an idea could result from it—of all this he had no idea, and could have no better idea, however rich and complex his experience might grow.

One may well say, then, that had Empiricism ended with Locke, it must have ended in a failure to resolve the paradox it had so hopefully attacked. But Empiricism did not end with Locke; and the reason it could develop beyond him was that it perceived at once how fatal to the hopes of an empirical science, how conducive to an absolute skepticism, must be Locke's admission that the "object" of empirical observation was a "something" of which one knew nothing now nor could ever learn more. "On this subject [writes Berkeley, reflecting on Locke's conclusions respect-

ing the substance of things] the skeptics triumph all that stock of arguments they produce to depreciate our faculties and make mankind appear ignorant and low are drawn principally from this head, to wit, that we are under an invincible blindness as to the *true* and *real* nature of things

We are miserably bantered, say they, by our senses and amused only with the outside and show of things The real essence, the internal qualities and constitution of every meanest object is hid from our view, something there is in every drop of water, every grain of sand, which is beyond the power of human understanding to fathom or comprehend " And indeed, Berkeley might have drawn a darker picture of the Lockian invitation to skepticism For if the "real essence" in all things is no other than the Lockian substance, then *not only is it forever hid from our view*, but, having no idea of it at all, we can have no better idea of it by virtue of any advance we may make along the ways of experiences "But," Berkeley hastens to add, "it is evident that all this complaint is groundless, and that we are influenced by false principles to that degree as to mistrust our senses, and think we know nothing of those things we perfectly comprehend "[3]

We have already seen on what grounds Berkeley's confidence in the soundness of the *observational method* rests it is the doctrine summed up in his famous formula "the *esse* of things is *percipi* " This is classic Empiricism's final contribution to the solution of the paradox that may be said to have called it into being The paradox is striking enough —the observer possesses *only mental facts*, *ideas*, yet he passes judgment on physical facts (e g the *length* of a path fallen through, the *time* taken in the fall), and from these latter arrives at the physical law that fits them If the physical facts neither are nor can become mental facts (as

Locke's *substance* neither was nor could be mental), then indeed "the skeptics triumph", there is no science possible But in presence of this paradox, Berkeleian Empiricism makes its hopeful suggestion What if the physical facts are themselves mental facts, what if the "observation" and the "thing observed" both belong to the one world of *ideas*? Then, indeed, "all this complaint would be groundless." And why should they not, why should not "observation" and the "thing observed" both belong to the world of ideas? Surely within this one world of ideas, there is room enough and means enough to distinguish between ideas that are "real things" and ideas that are "chimeras", between "ideas of sense" and "ideas of the mind's own framing" "The ideas of sense [i e those "real things" that cannot be "chimeras"] are allowed to have more reality in them, that is, to be more (1) *strong*, (2) *orderly*, and (3) *coherent* than the creatures of the mind but this is no argument that they exist without the mind They are also (4) *less dependent on the spirit* which perceives them yet still they are *ideas*, and certainly no *idea*, whether faint or strong, can exist otherwise than in a mind perceiving it" "For [and this is Berkeley's whole philosophy] our sensations be they never so vivid and distinct are nevertheless *ideas*, that is, they exist in the mind, or are perceived by it, as truly as the ideas of its own framing."[4]

From all this, it is plain that Berkeley conceives his most valuable contribution to the theory of "how we learn by experience" to have been the banishing of Locke's "vain word *substance*." That to which an idea has to be like, in order to be a "true" idea, can be nothing but another idea, for "an idea can be like nothing but an idea."[5] Remembering that for Berkeley "to have an idea is all one as to perceive",[6] and assuming that there is no essential difference

between what Berkeley calls "a perception" and what the experimenter calls "an observation," we see that Berkeley's conclusions have a very definite meaning for the experimental scientist of our day For the first time in history, Berkeley has formulated an important principle, in his own idiom he has expressed the thought that nothing can establish the truth or error of an observation save another observation Whereupon, one would be tempted to say, the "fact" said to be observed, when an experimenter records an observation, must be some other observation And, in fact, the experimenter does under proper conditions speak of a given observation as having been "confirmed" or "refuted" by a later observation, a manner of speech which suggests a likeness between the scientist's way of confirming an observation and the plain man's way of verifying a description For, does not the plain man test a description by confronting it with the thing described, and what is that but testing one observation by comparing it with another? And, would not common sense and science alike, be quite willing to call this procedure a facing of testimony with fact?

We see, however, that both for Berkeley and for the modern experimenter, this understanding of their common meaning would be a misunderstanding Neither would be prepared to admit that any observation falling within the limits of a finite experience could be the ultimate "fact" to which previous observations had looked forward as the object of their search What then? Shall we, with Hume and the later Empiricists, let *fact* follow in the wake of *substance*, to fall at last into the same void of nonentity? Shall we eliminate the *fact observed* from our vocabulary of science, and speak henceforth only of *observations of fact*?

Kant was the first to show how impossible it would be to follow out this suggestion For him, *observation* and *that*

which is observed were as definitely correlatives as *man* and
wife, *up* and *down*, *right* and *left*, of which, to void either of
meaning was to void the other also Of the many troubled
discussions running through Kant's Critical writings of the
relation between an *appearance* (Erscheinung) and *that
which appears* (Ding an sich), perhaps none puts his point of
view more succinctly than a passage which occurs in the
section of his *Critique of Pure Reason* entitled, "On the
ground for differentiating all objects whatever into pheno-
mena and noumena " "[It] follows naturally from the con-
ception of any appearance whatever that something which
is not an appearance must correspond with it, and so, if we
are not to have an eternal circle on our hands, the word
appearance [phenomenon] must be taken to indicate a
something which as immediately given is a sensuous per-
ception, but as it is in itself [noumenon] must be a some-
thing independent of [i e distinct from any] sensuous
perception "'' And, feeling the need of a word to distinguish
our conception of this *thing that appears* from our perception
of its *appearance*, Kant decides to call the former a *limiting
conception* (Grenzbegriff)

In spite of the habitually baffling quality of Kant's
phrasing, one may see nothing forced in an interpretation
that would sum up Kant's thought on the subject of the
appearance of things and the *things that appear* in the
single sentence the two terms, *appearance* and *reality*, are
correlatives, as such they are always distinguishable in
denotation, never separable in connotation And, as this
same sentence seems best to convey the sense in which such
terms as *observation of fact* and *fact observed* enter into the
familiar use of common sense and of experimental science
alike, our brief review of the history of the problem of
defining these terms brings us to the threshold of the

laboratory There, indeed, we must enter, if we are to continue in our design of examining into the theory of evidence that prevails in experimental science But if we do enter, we may expect to linger quite a while, for to have given sharper definition to a *long studied* problem is not to have solved it, and to have learned something of old failures that need not be repeated, is not to be assured of final success There is, however, one prospect that is both pleasing and promising opening up before us as we enter on this new field of observation and reflection we no longer need listen to what historic figures have had to say on the subject of appearance and reality, truth and error, we have only to watch what active men have been and are doing in order (as these men so often put it) to approach by way of closer and richer observation nearer and nearer to the goal of reality and truth

The present study proposes, then, to follow the experimenter into his workshop, and to watch what he does there in response to any one of those many questions of fact that are constantly being put to him For, respond he does in innumerable instances, first in acts (of measuring, counting, etc), and then in words dictated by the outcome of these acts Were it not so, there would be no such thing as experimental science It will then be for our reflection to *infer from what the scientist does* in response to such a question, the meaning that *question of fact* and *answer* hold for science

Of all experimental sciences, the first to come into being were those that found some way of responding to two questions of fact constantly demanding attention, and usually together the distance between two points, the angle between two lines The surveyor and the astronomer are the oldest of scientists, and no other experimental

sciences could have developed had theirs failed to mature;
for what could a science measure, could it measure neither
lengths nor times? All of which suggests that if one would
watch the scientist at work, one take for model the scientist
at work on one of those questions of fact that no science can
escape: questions of length and questions of angle. As a
whole, the problems of the two sciences dealing with these
matters are not separable: terrestrial and celestial surveying
are two branches of the same profession. But, in detail, the
earthly branch commonly deals with objects less remote
than the heavenly; let us then begin with such a common-
place detail of the surveyor's professional work.

It so happens, that an old *Report* (1854) of the U.S. Coast
Survey gives an unusual opportunity of watching and
studying an expert surveyor's way of meeting the question:
What is the angle formed at A by two light rays coming
from the distant objects B and C respectively? I.e., the
surveyor is asked to "determine" an angle BAC. In response,
we see him set up his theodolite over A, sight his telescope
on B and take a reading, then turn his sight to C and take a
second reading. The difference between these two angle-
readings "measures" the angle through which his telescope
has been turned.

To the layman, it may well seem that with this result the
question set the surveyor has been answered. For is not
the angle through which he has turned his telescope
exactly the angle formed at A by the two light rays from B
and C respectively?

But no; if this surveyor's "finding" were the fact he set
out to find, his search might discontinue; he might answer
the question put him, and therewith an end. Whereas, what
he actually does is to repeat the same measuring operation
again and again. The *Report* notes the number of measure-

ments taken in the survey it records to have been one hundred; all taken to 0.1, the "least reading" that the surveyor's instrument made possible. But these measurements did not all agree to their last decimal place. Had they done so, the layman might suppose the surveyor would at last have been persuaded to respond to the question put him in no uncertain terms: this one hundred times repeated figure must surely be the size of the angle he was asked to measure. On the contrary, such complete agreement of all his measurements, so far from encouraging the surveyor to return a confident answer to the question put him, would deter him from making any response at all, in the sense (later to be defined) in which science understands the word *response.* It is not accidental to this case of measurement, but common to all, that no experimenter will base a response to a question of fact on measurements that agree to their last decimal place. It sounds strange at the beginning but will prove reasonable in the end, to assert that while experimental science accepts no witnesses to matters of fact save measurements and enumerations, yet it will pronounce no verdict on their testimony unless the witnesses disagree.

Whereupon, seeing how far the one hundred witnesses our surveyor has gathered are from saying the same thing, and seeing how little reason there is for supposing any one of these witnesses to be more reliable than any other, the layman may well wonder why the experimenter should not have contented himself with the testimony of the first of the lot, and spared himself the trouble of gathering the evidence of ninety-nine others, no one of which is more reliable than the first. However, that trouble having been taken and the one hundred measurements made, it would be strange if the layman did not suggest taking the average

of the set as somehow more trustworthy than any one of the
items comprising it, though from what vague instinct of
scientific meaning this suggestion sprang, he might find it
hard to say How hard, is witnessed by his embarrassment
if asked to choose between two discrepant measurements
derived, respectively, from two sets of readings equal in
number and so similarly taken that the layman can
distinguish nothing to make the mean of either to be
preferred over the mean of the other

Here, the historian of experimental method could readily
comfort the layman, by showing him how little the layman
had himself to blame for this embarrassment Prior to the
year 1805, the scientist himself was in possession of no
general principle by which he might know whether, of
two sets of observations yielding discrepant means, he
was to accept either or neither, or perhaps to use both as
data for further calculation But the U S surveyor whose
work our *Report* is following was presented with none of
these special problems, and neither need we be Our interest
from now on is with the general principle without which not
only must such special problems remain unsolved, but also
the most general problem that can face any experimenter
who has taken a set of measurements in response to a
question of fact This general problem was formulated by
Legendre in the opening paragraph of an appendix to his
Nouvelles méthodes, "to get from measures given by
observation the most exact results that they can furnish "[8]
Evidently, this raises a question that neither our surveyor
nor any other experimenter who has taken a set of observa-
tions with a view to responding to any question of fact can
escape The answer, first published by Legendre, and
shortly afterwards confirmed by Laplace, Gauss, and others,
using different methods, is generally known as the *theory of*

least squares The theory of least squares, however, is itself but a special application of the theory of probabilities, so that from the time of Legendre it is on a calculus of probabilities that all experimentally controlled responses to questions of fact depend for their form and content

Continuing in the practice of watching without comment what the experimenter, here in the person of our surveyor, does, rather than listening to what he or another says, our next observation must be on the use he makes of the theory of probability now at his disposal in determining what, if any, response he should make to the question put him With the theory in mind and his one hundred measurements in hand, the first matter any experimenter would have to consider in determining his further procedure would be the degree to which these measurements fulfilled certain conditions imposed by the theory of least squares on any set of measurements to which the theory applies These conditions concern what is briefly called, the distribution of *errors* By *error*, in a set of direct measurements such as these angle readings, is meant the algebraic difference between any single measurement and the mean of the set For a set of measurements to be available as basis for any response the theory of least squares would justify, their errors must meet three requirements, namely [9]

  i   Small errors shall be more frequent than large
  ii Positive and negative errors of equal magnitude shall be of equal frequency
  iii No very large error shall occur

Now, the angle measurements of this surveyor were chosen for our present study precisely because the analyst of the U S Survey *Report* has preserved for us the data by which we may judge how close these measurements come to meeting the requirements stated, and therefore how fit

they are to serve as data for a response controlled by the
theory of least squares
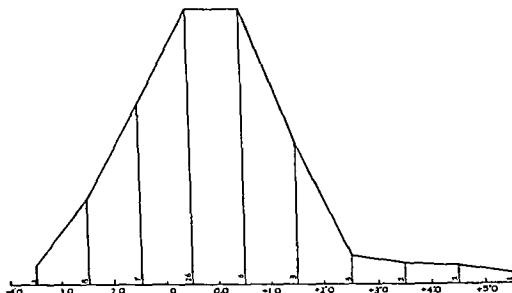
From U.S. Coast Survey Report 1854 p.91
Reproduced Merr man, Method of Least Squares 8ᵗʰ ed. 1911 p.15

In the accompanying "frequency polygon," the distri-
bution of "errors" occurring in a set of 100 angle-
measurements (taken to 0˙1) is graphically recorded In
the diagram, the abscissae (+1˙0, +2˙0, etc, −1˙0,
−2˙0, etc) represent seconds of arc in plus or minus
direction from mean (0˙0, the ordinates (26, 13, etc , 26,
17, etc) represent the number of variations from the mean
("errors") falling within the intervals of arc at whose
mean-point they are severally erected Thus, between 0˙0
and + 0˙1 the number of errors was 26, between 0˙1 and
+0˙2, 13, etc , between 0˙0 and −0˙1, 26, between −0˙1
and −0˙2, 17, etc

The fact that our surveyor carried his measurements no
further, but accepted their result as a datum for use in the

further course of the triangulation on which he was engaged, must for the moment be our evidence that he considered the "fit" of his frequency polygon sufficiently "good" to justify a practical response to the question he set out to dispose of Now, an experimenter who judges a set of measurements sufficiently well-conditioned to warrant a practical response based on their result, is always prepared, and, for purposes of publication, is always obliged to formulate his response in a way that will convey to another experimenter the "weight" of the evidence on which he has acted How, then, would our surveyor, having come to the end of his measurements, put into words or their equivalent, the information he supposes them to have furnished?

The answer, as dictated by the theory of least squares, requires the use of two terms defined by the theory itself the *most probable value* of the quantity measured [here, the *arithmetical mean* of the set of measurements], and the *probable error* of this most probable value As in the immediately following chapters the argument will make frequent use of the mathematical expression defining this "probable error of the mean," it will be well to agree on a standard symbolism for the numerous terms entering into this expression Let, then,

S_n	be a *set* of n measurements, $q_1, q_2, --- $
$\bar{S}_n$	,, the *arithmetical mean* of S_n
q_i	,, the ith measurement of the set S_n
q	,, a *"random"* measurement of the set S_n
$v_i = q_i - S_n$	,, the *variation* of the ith measurement from the mean
σ_n	,, the *standard deviation* of S_n
$p(q)$	,, the *probable error* of q
a	,, the abstract number 0 6745 $+$

Then

$$\sigma_n = \sqrt{\frac{\sum\limits_{1}^{n} v^2}{n-1}}$$

$p(q) = a \, \sigma_n$ (the probable error of a random measurement of S_n)

$p(S_n) = \sqrt{1/n} \, a \, \sigma_n$ (the probable error of the mean, S_n)

We see, then, that of the two expressions, "mean" and "probable error of the mean," in terms of which the theory of least squares would require our surveyor to formulate his response to the question put him, both are quantities whose numerical value is readily calculated from the set of measurements q_1, q_2, q_n It is to be noted, however, that $p(S_n)$ is a quotient whose calculation could be carried to any number of decimal places, yet its numerical value is significant only to the decimal place to which the measurements have been taken, i e , to the place whose order is determined by that of the *least reading* of the measuring instrument used

In terms of the symbolism agreed upon, the 100 measurements to $0˙1$ made by our surveyor yield the following figures

$$v^2 = 269˙0$$

$$\sigma = \sqrt{\frac{269˙0}{99}} = 1˙65$$

$$p(q) = 0˙6745 \times 1˙65 = 1˙11$$

$$p(S_n) = \frac{1˙11}{10} = 0˙1$$

In the sequel, wherever the discussion is general and the meaning clear, it will be convenient to write for $\overline{S}_n$, the single letter m, and for $p(\overline{S}_n)$, the single letter p With these two terms, m and p, at his disposal, the experimenter has all that is needed for the wording of a response to the question

put him We can, however, make this wording more concise, if we first introduce a new word defined in terms of these two The new word, not in current scientific use but as sharply definable as any term that is so, is *range* If the numerical value of a mean measurement is m, and that of its probable error p, we propose to define *numerical range* as the continuum of values extending from a lower limit m — p to an upper limit m + p In introducing the term *numerical range*, one simply gives name and definition to a symbol in universal use, (m ± p) To pass from *numerical range* to *metric range*, one has only to let the m and p of the numerical range become coefficients of [U], the unit of the scale in which the measurements are taken m[U] then becomes the mean, p[U], the probable error of the mean, and (m ± p) [U], *the range of that set of measurements*

For example, suppose our surveyor's mean measurement (not reproduced in the *Report*) to have been 60˚0′ 7′0, 15˚2 The probable error of this mean was seen to be 0˚1 The range of his set of measurements would then be written (60˚0, 7′0, 15˚2 ± 0˚1) The *magnitude* of this range (2 × 0˚1) is 0˚2, its *centre*, the mean measurement 60˚0, 7′0, 15˚2

In making use of the term *range*, just defined for the wording of his response, the experimenter is guided by the desire of conveying to a colleague who should read it, the maximum information of a certain kind concerning the experiment on which it rests The kind of information most useful to the scientific reader is such as will enable him to "weigh" the value of this experimental result against the value of others with which he may wish to compare it Of several wordings that might equally well serve this purpose, we suppose the scientist to choose the following general form

The magnitude in question is to be taken on evidence of n measurements to lie within the range (m ± p) [U]

In particular, our surveyor's response to the question put him concerning the angle BAC would be

The angle BAC (100 measurements) is to be taken to lie within the range (60°, 7′, 15″2 ± 0″1)

A response of this form, we take to be the most complete and informative result that can be obtained from a set of measurements taken in response to a question of fact And of responses in this form, we take the one just given in numerical detail, to be or to include all that Legendre meant by "the *most exact result* to be got out of the measurements [made]" Science, then, owes to Legendre and his colleagues its present knowledge of what most exact response it can return to any question of fact lying within a vast range of questions constantly addressed to the experimenter

But a *response*, however exact it may be—is it any *answer* to the question asked? The distinction seems unimportant, almost a quibble It may turn out to be one of the most important, most critical differences that a theory of evidence can have to consider

¹Locke, *Essay* II, i, 2 Condensed
²*Essay*, II, xxiii, 2
³*Principles of Human Knowledge,* CI
⁴*Principles,* XXXIII Arranged
⁵*Principles,* VII
⁶*Principles,* VIII
⁷*Kritik der reinen Vernunft,* A, Abridged
⁸*Nouvelles methodes pour la determination des orbites des cometes,* Paris 1805 Tr Reeger and Walker, Smith s *Source Book in Math* , p 516
⁹Merriman *Method of Least Squares,* 8th, ed , p 15

9. Question of Fact: the Unanswerable

IT WOULD BE IDLE TO SET OUT ON A SEARCH NOT knowing what one was looking for, it would be equally idle to seek the answer to a question of whose meaning one knew nothing But one could have caught nothing of the meaning of a question, did he not know one thing about the answer, namely the *form* this answer must take A surveyor, asked to determine an angle or a distance, *knows the answer to be some definite number of degrees of arc or meters of distance, all he has to do is to find which one of the infinity of numbers in our number system this number is*

The preceding discussion of a question *asking the size of a given angle BAC*, supposed itself putting a question that would be intelligible to a surveyor, an astronomer, or any other scientist to whom it was addressed, *no matter what three points in Nature might be chosen as the B and A and C locating the angle meant* But is this so? If B, A, C were respectively the centers (M, S, J) of Mars, Sun, Jupiter, would an astronomer, if asked to, set himself to measure the angles and sides of the celestial triangle MSJ? Certainly not, for no one in the course of human history could have supposed the question of angle here raised to have any one answer, and no one since Kepler's day has supposed any one of the sides of the triangle *MSJ* to have any one length And so, to our hypothetical question about this *celestial triangle, MSJ,* the astronomer would merely reply by pointing out that the question had no answer because it could have *no meaning,* unless we specified the exact date

in the history of the solar system for which we wanted this triangle to be determined

Not to make a long story of a simple matter, there is one and only one way to word a meaningful question about the angles and sides of triangles whose vertices are given in Nature and that is, to ask the size of the quantities indicated at some one moment of time Then and only then would the question call for an answer, and only so would it have meaning enough to permit of empirical response This would be equally true of a vast class of questions of which those concerning angles and distances of natural objects are but particular cases Throughout this class, only "dated" questions have sufficient meaning to call for an answer Our common way of speaking, preserved throughout the last chapter, seems not to observe this principle, it lets us ask a surveyor the size of a given angle without specification of date That a surveyor will accept and respond to such an undated question is a matter of convention, permissible where long experience in responding experimentally to dated questions of this kind leads to the conclusion that such questions receive the same response for quite a span of dates But even in responding to questions of this class, a condition involving dates is tacitly understood no one supposes the span of dates for which one can count on an invariant response to any question of this class to be unlimited, and so the convention of leaving them undated is only available when both he who asks and he who responds are by mutual understanding confining their discourse to a dated span within which the constancy of the quantity in question is considered an empirically safe assumption

And so, to make all matters at issue explicit, let the question put to the surveyor in the last chapter be dated, let us ask the size of the angle BAC at (say) 12 M, January 1,

1850 (since we know the survey analyzed in the *Report* to have been made in the early fifties), let us indicate the date for which a magnitude is required by the symbol t_1, and the required magnitude (in the present example) by the symbol BAC_1 Neither the method of responding nor the *response formulated in the last chapter need be changed to meet the change in form of question, only, we may now say without condition that the question*, "What is the size of BAC_1?" belongs to a class of questions of fact of which we knew the *form* of answer their meaning demands Thus, to the dated question put to our surveyor, any meaningful answer must take the form of the proposition

$$\text{The angle } BAC_1 \text{ is an angle of } \frac{560°}{k},$$

where k is some single number of the decimal number system A question whose meaning requires a single-valued answer (i e , an answer in the form The quantity asked about is nU, where U is the unit of scale in which the quantity is measured, and n a single number) may conveniently be called a *single valued question*

The last chapter ended with *a query as to whether the response returned by our surveyor to the question put him was to be considered an answer to that question* To resolve all doubts on this score, one has only to place side by side the form of answer this question demands, and the form of response it received

The answer (A) is to be the angle in question is an angle of $n°$

The response (R) is The angle measured is to be taken as an angle lying within the range $(m \pm p)°$

These two sentences will be seen to differ in all three of

their grammatical parts subject, predicate, and coupling verb, for

The subject of A is, *the angle in question*

The subject of R is, *the angle measured*

The predicate of A is, *an angle of n°*

The predicate of R is, *an angle lying within the range*
$$(m \pm p)°$$

The coupling verb of A is, *is*

The coupling verb of R is, *is to be taken as*

Now, we see it to be a matter of definition that the measuring process followed by our surveyor can never make available a response cast in any other form of sentence than the *imperative* form given in R It is no less a matter of definition that the question put to the surveyor admits of no meaningful answer save a *declarative* sentence, a categorical proposition, of the form given in A Finally, it is a matter of grammatical definition that no imperative sentence can be a declarative sentence, i e, no command can be a proposition From these definitional premises it follows of course that no experimental process analogous to the one our surveyor has employed can make available a response which is also an answer to a single-valued question of fact And if to have established this conclusion is to have shown it to be impossible, because involving a logical contradiction, for the experimental process to furnish an answer to any such question of fact as that put to the surveyor, then our reflections on the experimenter at work will have brought us to a conclusion that to many will seem disturbing, if not desperate For to the minds of many thoughtful men, the principle holds universally "a question that, if answerable, is only to be answered experimentally, and yet cannot be so answered, is a meaningless question" But these same thinkers would agree that a question as to the moment try

angle formed by two given light rays was, most certainly, a question that could have none but an experimentally-founded answer If then, the theory of experimental evidence should itself close the logical possibility of answering the question here asked, the proponents of the general principle stated could draw but one conclusion All questions of the class to which that put to our surveyor belongs, a class to respond to which the experimenter devotes most of his time and effort, are meaningless questions

Now, *the caution of those who distrust the meaningfulness of unanswerable questions* has so often justified itself by exposing the faulty premises of specious skepticisms—skepticisms likely to become premises of bewildered mysticisms, that one cannot afford lightly to dismiss any unexpected embarrassment to which that caution may give rise And so, the matters of next consideration in connection with the class of questions here under discussion we take to be these Are such questions answerable? If not answerable, are they meaningless? If not meaningless, what in their case do the correlatives, *question and answer,* mean?

Whatever may be the experimentalist's way of meeting these problems, he must begin by accepting the fundamental premise from which they arise He admits then that not only the question put to the surveyor, but most if not all questions of fact, if answerable at all, are only to be answered on the evidence of experiment He further *admits that the only contribution this surveyor's experiment has made looking toward an answer is to establish a certain range of values* $(m \pm p)°$, which range is to serve as the predicate of a response, conveniently represented by the symbol $R[m \pm p]°$ He further admits that the only change further experiment could effect in this response is a change in the values fixing the center and magnitude of the range let us

say, a change from a response $R[m_1 \pm p_1]°$ to a response $R[m_2 \pm p_2]°$ Finally, he admits that the only meaningful answer the question thus responded to could have is a proposition of the A-form, whose predicate is to be a single value let us represent this answer by the symbol $A[n]°$ In presence of all these agreements and admissions, no one could deny the logical contradiction involved in supposing that for some value of $(m \pm p)°$, the response $R[m \pm p]°$ would become the answer $A[n]°$ And if this settled the question, we might set down at once the conclusion that there are at least some questions of fact, only to be answered by experimental method, to which experimental science can give no answer, that experimental science, therefore, must recognize the existence of a world of unanswerable questions of fact But if we are right in supposing question and answer to be correlatives, of which neither can have a meaning if the other have not, is not this world of questions that have no answers as difficult to live with as would be a world of husbands that had no wives, or of parents that had no children?

Now it may be that there is no final escape from the problem of meaning arising out of the admission to our world of thought of a world of questions of fact that have no answer and, therefore, no meaning But if so, it must be on some firmer ground than that suggested by the argument of the last paragraph For one can imagine a logician pointing to a possibility this argument has overlooked He would agree, of course, that for no value experiment could establish for the quantity $(m \pm p)$, could a response $R[m \pm p]$ become an answer $A[n]$ But he would point out that this does not exclude another possibility, namely, the possibility that a response of the form $R[m \pm p]$ should, for some value of one or both of its variables, become an order

commanding the scientist to accept as true a judgment of
the form A[n] Suppose, e g , the surveyor, at the conclu-
sion of some sufficiently prolonged and refined set of
measurements, to have established for the predicate of his
response a range $(m \pm 0)°$, 1 e to have gathered a set of
measurements the probable error of whose mean was zero
Then would not experimental theory authorize the surveyor
to return to the question put him, the response R$[m \pm 0]°$?
And though $(m \pm 0)°$ is no less a range of values than is
$(m \pm 0\ 1)°$, and though the n of the required answer A[n] is
not to be a range but a single value, yet arithmetic lets us
find a solution for the n of the equation $m \pm 0 = n$,
namely $n = m$ Now, as to have found a response to a
question of fact is to have received a command issued in the
name of science itself, and as the scientist is by definition
one who owes all obedience to this law giver, there is
nothing for the scientist to do, but do as he is ordered Our
surveyor, then, supposing him to have established a zero
range, will have been commanded in the name of science
"to take the angle measured to he within the range
$(m \pm 0)°$" This he can do in but one way, namely, by
formulating and accepting as true the categorical proposi-
tion The angle measured is m° So we may say in general
that to have established a zero range is to have arrived
experimentally at a *proposition dictating* response to the
question asked, and *in accepting this proposition as true, is*
he not accepting it as an answer to this question?

Certainly, not yet, for though the response dictated
proposition thus accepted *has been brought into identity*
with an answer to the question asked *in two of the three*
respects in which a response of the form R$[m \pm p]$ differs
from an answer of the form A[m], there remains that third
difference not yet shown to have been diminished Yes, the

response-dictated proposition and the answer are identical
in predicate-term and coupling-verb, but what of their
respective subject-terms? Nothing that has yet been said
implies the identity of the subject of the required answer
(the quantity asked about) and the subject of the dictated
response (the quantity measured) Whether to have estab-
lished a response of the form $R[m \pm 0]$ is or is not to have
eliminated the difference between these two subject terms
remains a matter for later investigation But though we do
not yet know the establishing of a zero range to be a
sufficient condition to the answering of a single-valued
question, we may well accept it as a *necessary* one And as
this makes a vast class of questions of fact, to which the
question put to our surveyor belongs, unanswerable *unless*
experimental method can establish a zero-range in response
to such questions, it becomes a matter of primary import-
ance to know whether or not it is possible for experimental
science to establish a zero range For if not, it would appear
that either we must look elsewhere for any meaning the
answer to such questions may have, or we must abandon all
hope of making the questions themselves meaningful

Does, then, the experimentalist theory of evidence
recognize the possibility of establishing by experimental
methods a zero range in response to a single valued question
of fact? The answer is a decided *no*, but the reasons for this
no must be carefully examined, if we are to accept its veto
as final with all the consequences that follow from it

To put the argument in briefest form, it will be well to
recall certain matters referred to at the outset, and to
introduce certain convenient terms these matters permit us
to define Thus, introducing Legendre's theory, it was
noted that not every set of measurements, however care-
fully taken, is competent to yield a range that may be used

as predicate of a response   To be sure, any set of measure-
ments, or for that matter, any set of arbitrary numbers,
must, on application of the range-finding formula, yield
arithmetical values for m, p, m ± p, but these values would
not have the meaning required of them, if (m ± p)U is to
serve as predicate of an empirical response, unless the set of
measurements *sufficiently* conformed to the conditions of
frequency-distribution previously specified (p 105)   Whether
a given set does or does not sufficiently conform to these
conditions has so far been left to the judgment of the
scientist, based on his inspection of a frequency polygon
Later,[1] we shall see how this graphic test may be replaced
by a numerical criterion, less personal and more discriminat-
ing in its pronouncements   Without, for the moment,
formulating such a test, we may give it a name   let us call it
a test of the *responsiveness* of a set [1a]   It will then be con-
venient to call a set withstanding this test of responsiveness
a *responsive set*, and the method of measurement by which
a responsive set was taken, a *responsive method of measure-
ment.*   Our question, then, becomes   Can any responsive
method of measurement be found, such that a finite set of
measurements taken by this method will yield a zero-range?

     To this, experimental theory may return a two-fold
answer
(1) It is possible to gather a set of measurements for which
the calculated range would be (m ± 0)U, in which 0 would
be that zero of arithmetic for which the equation holds,
m U ± 0 = m   But, such a set would not be responsive
(2) It is possible to gather a responsive set of measurements
yielding a range (m ± [0]$_v$)U in which [0]$_v$ would be a
quantity expressed as zero to its last (the vth) decimal place,
where v is the decimal order of the least measure *to which*
the measurements had been taken   But such a "zero"

would not be the zero of arithmetic, and the equation $(m \pm [0]\nu) = m$, would not hold

The first of these two possibilities would be realized by, and only by, a set of measurements that agreed to their last recorded decimal place For such a set, the value of all the deviations, v, would be o, and so the numerator v^2 of the fraction $\sqrt{\Sigma\, v^2/n - 1} = \sigma$, entering into the expression for p, would be zero Reference to the formula for p (p 106) will show then that for any finite number of measurements the value of p yielded by the set would be an arithmetical 0 The set would indeed have yielded a calculated range $[m \pm 0] = m$, but such a set would leave unfulfilled the very first requirement of any responsive set, for, if small errors must be more frequent than large, it follows that errors greater than zero must occur [2] On the evidence of such a set, no response at all could be returned to the question asked

As to the second possibility, the theory of least squares establishes a theorem (later to be discussed), to the effect that any responsive method of measurement not only can, but in order to remain responsive must, for a sufficiently large number of measurements, yield a range $(m \pm [0]\nu)$U, where ν is the order of the last "significant" decimal place, as determined by the "least" reading Such a range is, by definition, available as predicate of a response, but the scientist reading this response would interpret the range, within which the quantity measured must be taken to lie, to be a range narrower than $(m \pm 0\ 0 \qquad 1\nu)[U]$, indeed, but broader than $(m \pm 0\ 0 \qquad 0\nu)[U]$, and therefore broader than $(m \pm 0)[U]$ Moreover, he would know that once a given method of measurement had yielded a set whose range was $(m \pm [0]\nu)[U]$, the value of the range beyond the νth decimal place could not be

definitely determined by that method of *measurement*, until the method had so improved its technique as to enable it to take its measurements to a least reading of higher decimal order One sees then that (what we may call) an *empirically zero range* is far from being, and can never become, an *arithmetically zero range*, for which alone the numerical equation, $m \pm 0 = m$, can be taken to hold

Since the argument just completed applies directly to the question put to our surveyor, it establishes the conclusion that there are at least *some* unanswerable questions of fact But the argument is equally though less directly conclusive for a whole class of questions to which the surveyor's belongs To this class of questions, we may give the name *metric*, to distinguish them at once from another class to which our argument applies neither directly nor indirectly This class of non-metric questions we shall call *statistic* A metric question is one to which any experimental response demands for its initial data, *readings*, each reading being a count of units in one or another of the continuous scales A statistic question is one to which any experimental response demands for its initial data, *numbers*, each number being a count of discrete units, i e , units fulfilling some common condition other than that of combining to form a continuous scale Whether or not statistic questions are, some or all of them, unanswerable, is a matter whose discussion may be postponed, not to interrupt the course of our thought on metric questions For when we consider the vast variety and historic importance of questions belonging to the metric class, the conclusion that *no question of this class is answerable* gives us enough to think about for the moment

How extensive, how important is the class of metric

questions appears the moment we reflect that it includes all questions to which an experimental response depends on measurement for its data. For every measurement is a mathematical function of readings. These functions may range in complexity from a simple difference between two angle-readings, or two length-readings, to forms whose possible complexity is only limited by the ever-increasing capacity of the human mind to formulate and compute from them a measure of the quantity a given metric question demands for the predicate of its answer. The *dimensions* of the quantity that a metric question would require as predicate to its answer serves to place that question in a scheme of classification that puts in the same class questions requiring predicate terms of the same dimension, i e., expressible in terms of the same unit. Among the questions constantly being put to the scientist of our day, we commonly recognize, besides questions of angle and length, questions of time, mass, temperature, charge, pole strength. With these questions concerning quantities of one dimension, are to be classed an endless variety concerning quantities whose dimensions are functions of any number of these primary dimensions: velocity, acceleration, mass, force, energy, etc. To some questions of each of these classes the experimenter of our day has found ways of responding, to others he is hopefully seeking ways of responding, while the scientist of the future may be expected to have found ways of responding to metric questions that today are not only beyond reach of our response, but beyond thought of our asking. In short, experimental science from Galileo's day to this has lived in faithful observance of Galileo's conception of its function "to measure all that is measurable, and to make measurable all that is not." And so it may be expected to live for all its future.

The picture into which this survey of the ever-increasing number and diversity of metric questions of fact to which science has found ways of responding has entered, is challenging enough to thought For it is demonstrable from the very theory of measurement itself that *no one of these ways of responding to, can ever develop into a way of answering the question asked* No metric question is or can ever become answerable But if, as our "cautious mind" contends, experimentally unanswerable questions of fact are meaningless, where does this leave us? Even should all non-metric questions (among them all statistic questions) prove answerable on experimental or any other reliable evidence, where does a theory lead us, that would make the labor of all the observatories and laboratories of the world *no more than a tribute to human folly?* For what but folly can we call a vast absorption in the study of meaningless questions?

Before we let these reflections drive us back into that old but persistent skepticism, which sees in all that science has achieved or could achieve nothing more than what Cusanus called a *docta ignorantia*, it will be well to recall certain matters of history To recall, namely, *that whatever we may say of empirical science, formal science—notably mathematics—has long known itself to be faced with an infinity of unanswerable questions which yet the formalists have persistently refused to accept as meaningless, and for which they have finally found a well defined meaning* Is there or is there not, we ask ourselves, a sound reason for admitting *the unanswerable questions of formal science to be meaningful, yet maintaining the unanswerable questions of empirical science to be meaningless?* The matter seems to be important enough to be worth *considering with some care and at some length*

¹Editor's note: Singer had intended one of the chapters referred to in the Introduction to cover modern statistical methods of estimation.

¹ªIn current technical discussions, our "test of responsiveness" is generally referred to as a "test of randomness."

¹Editor's note: Evidently meaning that large errors occur with non-zero frequency. In the original manuscript, the axiom that errors greater than zero occur, was included. It was crossed out in the revised manuscript, because apparently the author believed it to follow from axiom (see p. 103).

10. Answer to Question of Fact: Predicate Term

THE STORY OF THE UNANSWERABLE QUESTION OF A formal science, with the problem of meaning it raises, goes back to Pythagoras' day, when it was first discovered to the Greek mind that what was then taken to be the entire range of definable numbers held none whose square was equal to 2 For definable numbers were at that time limited to those we now call *rational numbers* (i e , numbers that can be expressed as a ratio of two finite integers), and it is true enough that among all these numbers there is none whose square is not either greater than or less than 2 That the discovery was foudroyant, we know, it was held (for reasons of the day) to be so dangerous to human welfare that Hippassos of Metapontium (as legend has it) was put to death for having divulged this fatal secret to the crowd And even to the young learner of today, who is as restricted as were the most learned of Pythagoras' day to the domain of rational numbers, it always comes as a shock to discover that while it is easy to see that the number whose square is 4 is 2, it is impossible to find a number whose square is 2

Of the two questions What is the number whose square is 2? What the number of degrees in [BAC]i? both are known to be unanswerable Yet the mathematician of today is far from supposing the former question to be meaningless Not even our "cautious mind" pretended that *all* unanswerable questions were meaningless, and we did not so represent him His meaningless questions were confined to those

"that could be answered in no other way than by experiment, and could not be so answered."

What then, is the difference between the two kinds of unanswerable, the one addressed to a formal, the other to an empirical science, that the one should be meaningful, the other meaningless? We may best define the difference between the two and judge of its significance, after having noted their many points of likeness.

Consider first the kinds of question that the mathematician accepts as meaningful. They include:

1. Questions known to be answerable: e.g., What is the number whose square is 4?

2. Questions known to be unanswerable, but susceptible of a response differing from an answer-dictating response by a difference less than any given difference: e.g., What is the number whose square is 2? (Let us, for brevity and without change of meaning, call this class of the meaningful, questions susceptible of progressively accurate response.)

3. Questions (if any such there be) not known to be excluded from classes 1 and 2.

Meaningless, then, are only such questions as are known to be neither answerable nor susceptible of a progressively accurate response, e.g., What is the *rational* number whose square is 2?

Since no metric question put to the experimenter can belong to the first of these three classes of meaningful question—the answerable—any search for analogy between the meaning of formal and the meaning of empirical questions must begin with the second class: questions known to be unanswerable, but susceptible of progressively accurate response.

What meaning, then, does the mathematician attach to our question concerning the number whose square is 2?

Or rather, since the meaning to be given a question depends on the meaning to be given its correlative term, the answer, how does the mathematician define the unattainable answer to this question? For ease of comparison between responses returned to mathematical and experimental questions, respectively, let us put the information conveyed at the end of each step taken in extracting the $\sqrt{2}$ in a special way The sequence of numbers obtained by the process of extraction begins with the numbers 1, 1 4, 1 41,   The sequence of responses that these results make possible begins with the responses The number whose square is 2 must be taken to lie within the range 1 5 $\pm$ 0 5, 1 45 $\pm$ 0 05, 1 415 $\pm$ 0 005,   Each of these responses, which the mathematician returns to a question respecting the value of a certain number, is identical in form with the response the surveyor returns to a question respecting the size of a given angle Expressed in the idiom of ranges, an idiom common to the responses of formal and experimental science, the points of present interest established by the theory of irrational numbers for the particular irrational $\sqrt{2}$ are the following

(1) By no process can we ever come closer to knowing the number whose square is 2, than by continuing the operation of extracting the $\sqrt{2}$ to a decimal place beyond any attained decimal place

(2) By this process, no mathematician, of however remote a future, can have come to know more respecting $\sqrt{2}$ than that it must be taken to lie within the range $(m \pm p)_\nu$ established by the last (νth) decimal place to which extraction will have progressed at that date

(3) Therefore, the $\sqrt{2}$ is unknowable, the question that asks for it is unanswerable

(4) But though unknowable, the $\sqrt{2}$ is not indefinable,

although unanswerable, the question asking for the number whose square is 2 is not meaningless For the answer, which the process of extraction can never *find*, this process enables us to *define* in a way we must now consider

First, as to that zero range to which any method of responding must give meaning, if it is to find meaning for an answer-dictating response requiring this term for its predicate, here and throughout, reasoning into which the term *range* enters may be more briefly worded if we agree to restrict the term *reduction of range* to a limited use Let us, namely, understand one range to be a reduction of another, if and only if the one is of less magnitude than and lies within that other With this understanding, elementary mathematics lets us say that, if $(m \pm p)_\nu$ is the range established for $\sqrt{2}$ by extraction to the νth decimal place, then for increasingly high values of ν, the range established to any lower value of ν will be reduced to a range of a magnitude exceeding zero by an amount less than any given amount, while all numbers included in a range calculated to a higher value of ν will have been included in all ranges calculated to a lower value of ν Under these conditions, the mathematician considers himself to have established the proposition that the magnitude of the range $(m \pm p)_\nu$ is convergent for increasing values of ν, while its mean, m, approaches a limit which we may symbolize by μ He would doubtless accept as his final definition of this zero-range, the form of words best suited to our present and future needs The range, $(\mu \pm 0) = \mu$, is the *limiting conception* of the range towards whose magnitude, zero, the magnitude of $(m \pm p)_\nu$ converges, and whose mean (μ) is the limit approached by the mean of $(m \pm p)_\nu$ as ν increases indefinitely

Our second step toward defining the answer to an un-

answerable question of formal science is a mere matter of elimination It will be remembered that *the response to a metric question of fact differs from an answer not only in the predicate but also in the subject term* For while the subject of the answer is, "the quantity asked about," the subject of the response is, "the quantity measured " However necessary it may be in considering responses to metric questions to hold open the possibility that the quantity measured may not be identical with the quantity asked about, no such possibility would have meaning (save as resulting from a misunderstanding of the question asked) for responses to the formal question concerning the number whose square is 2 The subject of the question asked and the subject of each response returned are definitionally identical

Our third and last concern is with the coupling verb, which in both formal and empirical response has the same imperative form, "must be taken " For though the wording of the command is identical, yet the authority issuing the command may differ in the two cases, and differ in just the way that would deter our "cautious mind" from attaching the same meaning to the formal and to the empirical unanswerables For the sake of future comparison, we may establish at once the authority that gives its sanction to the *must* of a formal response This authority lies in the postulates of a science that the scientist himself creates for his needs, and the *must* so authorized expresses a formal necessity of the scientist's own creation Already one foresees that the reasons prescribing the limits within which $\sqrt{2}$ "must be taken to lie", cannot be identical with those dictating the range within which [BAC], "must be taken to lie " A formal science whose responses are completely determined by rules of its own *making*, cannot have a complete analogue in an empirical science, whose responses

are at least partly controlled by data gathered from its *findings* Whatever the inferences ultimately to be drawn from this general consideration, it lets us at once ask ourselves a quite definite question From the reasoning that establishes the method by which $\sqrt{2}$ is to be extracted, it is evident that the range $(m \pm p)_\nu$ includes all *possible* values of a number whose square is 2 Are we to suppose that the same formal necessity restricts the real size of [BAC], to the range $(m \pm p)$ obtained by applying a calculus of probabilities to n measurements of that angle obtained from angle readings? One anticipates a negative answer, but leaves the question for later consideration

At the end of these three steps, we are in position to state quite clearly the mathematician's definition of the correlatives *question* and *answer* for a particular case in which the question is known to have no findable answer e g , the case in which one asks for the number whose square is 2 The answer to this question is the proposition commanded to be formulated by an answer dictating response This answer-dictating response is the *limiting conception* of the response approached by the sequence of responses R $(m \pm p)_\nu$ as ν is increased without limit This limiting conception is well-defined, because and only because it is demonstrable from the postulates of arithmetic that (1) the predicate term of response $R(m \pm p)_\nu$ approaches the limiting conception of a *zero-range*, since with increasing values of ν its magnitude is convergent and its mean approaches a single-valued limit, (2) the subject of every response is identical with the subject of the question, and as this same term is to serve as subject to the answer, question and answer must have an identical subject, (3) the *must* of the coupling verb of every response has the force of formal necessity, and as this same verb is to be the coupling verb of the answer-dictating

response, the compulsion on the mathematician to formulate the proposition so dictated is one that he cannot escape without falling into the logical self contradiction of being no mathematician. But the proposition thus dictated is no other than that correlative of the question asked, the answer To define, not to find, this answer is a necessary and sufficient condition to its correlative, the question, being classed as meaningful

The meaning that the mathematician thus attaches to the question as to the number whose square is 2 shows it to fall within the second of our three classes of the meaningful, namely, questions known to be unanswerable, but proven to be susceptible of a progressively accurate response. To this same class the mathematician would assign every question proven to require for its answer an irrational number, i e , a number not otherwise to be defined than as the limit of an infinite series of *progressive approximations*, an expression that the mathematician with his ample equipment of well-defined terms would have no difficulty in defining, and of which our series of progressive extractions of $\sqrt{2}$ offers the most familiar special case

The third class of questions accepted by the formal scientist as meaningful, has for the modern arithmetician little more than historical interest, *though its importance to the empirical scientist may turn out to be of quite another order* It is only a matter of history that the mathematician of today recalls a time when his predecessors could neither find nor define *as a limit* the answer to such arithmetical questions as arose out of the geometrical problems of "duplicating the square," "duplicating the cube," "squaring the circle," "trisecting the angle," etc Yet, as the mathematicians of that day were equally without proof that the finding or defining of such an answer would be inconsistent with the

postulates of their science, they refused to abandon these
questions as meaningless: the questions retained for ancient
science such meaning as would place them in the
third of our three classes of meaningful questions. And in
this conservatism the mathematician of early days did well;
for later history shows his descendants to have found, in one
case after another, the series whose limit defined the answer
to the question asked. But this very result also shows the
early filling of our third class of meaningful questions to
have been but transient; all historic examples of such
questions have since been shown to fall either into the
second class of the meaningful (e.g., what number $= \sqrt{2}$?)
or into the class of the meaningless (e.g., what rational
number $= \sqrt{2}$?). True, questions once forced out of the
third class of the meaningful have always been and, so far
as one can see, will always be replaced by others, but there
is no reason to suppose that any *one* formal question will
ever be shown to lie *permanently* in this last class of the
meaningful. If, then, it should later appear that no empiri-
cal question of the metric order could ever be found to lie
in any class of the meaningful other than the third, this
might explain many things. It might point out the very
difference between the unanswerables of formal and the
unanswerables of empirical science that has led our "cauti-
ous mind," rightly or wrongly, to accord a meaning to the
former, and to deny it to the latter class of questions. It
might even go far toward explaining the vastly greater
difficulty the mind of the ages has found in defining such
reality or such *truth* as is to be sought along the ways of
experience, than in defining such *validity* as is to be deduced
by formal logic from the postulates of formal science.

But, of course, there could be no ground for drawing any
distinction between the questions put to our mathematician

and to our surveyor respectively, so long as every proposition that held of one held of the other class of questions Let us then follow our original suggestion and consider first of all what we can find of analogy between the empirical and the *formal* questions, for only if and when such analogy breaks down can there be any *occasion to follow* to their effect upon the matter of meanings the implications of such breaks in analogy as may then appear

At the outset, there appears to be no reason why the formal scientist's definition of the meaningful and the meaningless, together with his classification of the former, should not be equally satisfactory to the empirical scientist It is only when we come to examine into the denotation— the filling—of the classes thus defined that a difference between the distribution of formal and empirical questions among the three classes calls for attention Thus, the formal scientist recognizes an infinity of mathematical questions to be meaningful in the first of our three senses all these questions are answerable Whereas the empirical scientist, whatever he may later conclude respecting *the classification of non metric questions of fact*, has definitely shown that this first class of the meaningful must remain forever empty of metric questions, not one such question can ever be answered Still, this condition of being unanswerable no more robs these *empirical questions of meaning* than the identical condition robs of meaning the infinity of formal questions *whose answer demands for its predicate term an irrational number* May we not then accord to these empirical, as we accord to those formal questions, membership in the same class of the meaningful, namely, the second of our three classes, the class of questions for which no answer can be found, but for which an answer can be well defined as a limiting conception?

The theory of metrics that bears on this question is doubly interesting in that it lets one see how close we may come to, yet how far we must keep from, returning an affirmative answer. To follow its reasoning, we cannot do better than return to our surveyor, whom we left on the point of offering a response of the form R $(m \pm p)^\circ$ to a question of angle-size. He, or any scientist who responds in this way to a metric question, does so on the assumption that the set of measurements establishing his range conforms sufficiently to the three conditions (p. 103) imposed by the theory of least squares on a responsive act of measurements. So far, no other test of this assumption has been offered than such as might be furnished by a frequency-distribution polygon, the like of that constructed by the analyst of our surveyor's results (p 104). Such a graphic test of a set of measurements is illuminating to the extent of giving one a fair idea of the evidence that *has* been accepted as sufficient to show a given set of measurements conformal to these three conditions, but is this evidence sufficiently sound to satisfy the conscience of the scientist himself?

We see at once why it cannot be. Our graphic test leaves it to the personal judgment of the individual scientist to decide whether or not a given frequency-graph is a "sufficiently good fit" to the mathematical curve which *represents the perfect fulfilment of the three conditions to which* a responsive set of measurements should conform. Should two experts, judged to be of equal competence, differ in their opinion on the subject, there is no way of referring the issue to a higher court for an authoritative decision. Could we, however, deduce from the theory of least squares an arithmetical test of responsiveness that would reduce the test of "sufficiently good fit" to a question as to whether one

of two numbers were or were not greater than the other, the scientist would be relieved of dependence on individual opinion, a dependence from which all science seeks to escape

In considering how far an arithmetical test of responsiveness is to be derived from the mathematical theorems of least-square theory, it is well to remind oneself that no theorem of that theory can apply strictly to a finite set of measurements, for the simple reason that the measurements for which the postulates of the theory are assumed to hold are taken to be indefinitely numerous What one means, then, by saying that a finite series of measurements meets the requirement of a given theorem is that the series shows no characteristics which, if displayed by a series of greater length, would prevent that series from being accepted as having a certain property—here, responsiveness Only in this sense could we *deduce* from the postulates of least-square theory a condition which must be fulfilled by a finite series of measurements if it is to be accepted as a set "sufficiently" responsive to justify the scientist in proceeding to calculate from it the most probable value of the quantity to be measured and the probable error of that value But in this sense we can deduce from the premises of that theory the following conditions

Let a series, S, of n measurements fall into two subseries, S′ and S″, comprising respectively the first m and the remaining (n m) measurements of S, and for n/m, let us write k Then, if S is to be accepted as a responsive set, σ'/σ'' must lie within a certain range $(1 \pm p(\sigma'/\sigma''))$ [1] Formulas to be found in textbooks on the subject show this range to be susceptible of expression in the form of an algebraic function $(1 \pm f(k,n))$ in which k and n are the only variables The properties of this function of immediate interest to us are three

(a) Since k and n are known numerals, f(k,n) is an abstract number, and as the value of σ'/σ'' is also an abstract number, the question as to whether the set S meets a condition necessary to its acceptance as a responsive set is answered by comparing the relative magnitudes of two abstract numbers So far, then, as our test of responsiveness has developed, it has realized the desideratum which introduced the present discussion the question as to whether a series of measurements does or does not meet a condition necessary to its acceptance as a responsive set, is no longer *left to individual judgment as to the "sufficiently good appearance"* of a frequency-distribution graph, but is determined by a comparison of two abstract numbers, a comparison whose decision is no longer a matter on which two equally competent scientists might differ without possibility of finding which was right, which wrong

(b) It can be shown that, for a constant value of k, f(k,n) is convergent for increasing values of n, that is, for k = constant, limit n→ ∞ of $(1 \pm f(k,n)) = (1 \pm 0) = 1$ This deduction from the necessary condition imposed upon a responsive set of measurements is sometimes given the non-mathematical phrasing The standard deviation of a responsive set of measurements tends toward constancy as the number of measurements increases without limit

(c) Although the mathematical deduction of a third implication is beyond the scope of the mathematical resources of so brief a study as the present, a certain use of the preceding theorem may serve to make its corollary a result to be expected Recalling the expression (p 106) for p(S̄), it will be seen that, if, for the standard deviation σ, we wrote a constant, the equation would become $p(\bar{S}) = (1/\sqrt{n}) \times$ (constant), making $p(\bar{S})$ convergent for increasing values of n Now, the standard deviation is not, of course, constant,

but it does "tend toward constancy" with increasing values of n It is, then, not surprising to find that mathematical deduction arrives at the same result that would follow if σ were a constant,[2] namely, in a series of n measurements that constitute a responsive set for all values of n, the probable error of the mean is convergent for increasing values of n This theorem is sometimes given the non-mathematical phrasing *The probable error of a response based upon a set of n measurements tends to vanish with increasing values of n*

With this theorem, the analogy between the propositions that hold for the predicate of the response to be returned, whether to a question as to the number whose square is 2, or to a question as to the number of degrees included in the angle [BAC], is drawn as close as it can be at the present stage of our development To reduce the analogous propositions that hold for the responses to formal and to empirical questions respectively, to a single proposition that holds for both equally, we may introduce a few easily definable terms Let us understand the term *operation* to cover the processes, different in execution, identical in result, by which the formal and the empirical scientists, respectively, raise an approximation of the νth order to one of the (ν + 1)th order, the term *range of permissible values* to apply to the expression (m ± p), whether it refer to the range of all possible values (as in formal science) or to the range of all sufficiently probable values (as in empirical science), the term *necessity test*, to abbreviate the phrase, "a test to respond to which is a necessary condition to a range (m ± p) qualifying as a range of permissible values " Then we may say of a response R(m ± p) established by a set of n operations responsive to a necessity test that, for increasing values of n,

(1) The probable error (p) is convergent,

(2) The range of permissible values (m $\pm$ p) approaches the limit (m $\pm$ 0),

(3) The predicate term of a response of the form R(m $\pm$ p) approaches the limiting conception of the predicate-term of an answer-dictating response of the form R(m $\pm$ 0),

With this conclusion before us, shall we not say that the predicate term of answers to both formal and empirical unanswerables are to be identically defined as limiting conceptions approached by an unlimited series of responses meeting the necessity-tests imposed by formal and metric sciences, respectively? If so, there remain but two of an original triad of questions to be answered, if we would know whether the method of defining the correlatives, *question* and *answer*, available to formal science be not equally available to metric science, when faced with unanswerable questions The three differences, it will be recalled, that separate any attainable response and any meaningful answer contrast the multiple-valued predicate inevitable to a response with the single-valued predicate demanded of an answer, the verbal designations of the subjects of response and question, respectively, the imperative form of the coupling verb ("must be taken") of the response, with the indicative form of the verb ("is") required of an answer

The argument just concluded has placed no obstacle in the way of supposing the predicate terms to unanswerable questions of formal and of metric science to be susceptible of identical definition as limiting conceptions, no obstacle, then, in the way of turning to the second of our triad of questions that which would examine into the relation between the subject terms of question and answer in unanswerables, both formal and empirical Yet no sooner does

one open a discussion of this topic then he finds himself in the presence of a situation which forces his attention back to the topic we may have thought dismissed that of predicate terms. The next chapter will promptly offer an example, as simple as any that can be found, of conditions under which a series of measurements may respond perfectly to our necessity-test, yet be affected with a "systematic error" that robs the series of all claim to being a responsive set, and therewith robs the mathematical expression $(m \pm p)$ calculated from it of any claim to be a range of permissible values, such as might serve as predicate-term of a response of the form $R(m \pm p)$ The example is one of a large and endlessly varied class, by no means limited to situations in which the identity of the subject terms of question and answer is in question, yet it will be convenient to postpone study of the general case till we shall have examined this simple example of it

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[1]Editorial Note σ and σ^* being the standard deviations of the two subsets, see p 106 The test would usually be carried out by computing $F = (\sigma)^2/(\sigma^*)^2$, and referring to tables to be found in most elementary statistics text books, using a "suitable" significance level Singer has used the notation σ for the sample standard deviation, instead of the more usual s

[2]Editorial Note Since σ is the sample standard deviation it does vary from sample to sample The existence of the "true" standard deviation is the problem Singer is discussing

11. Answer to Question of Fact: Subject Term

LET THE PRESENT CHAPTER OPEN WITH A QUESTION: what kinds of error may result from mistaking an object measured for an object asked about? A serviceable classification of such errors suggests itself, dividing them into (1) errors that *do not* and (2) errors that *do* depend for their detection on the accuracy of the measurement made.

(1) To the first class belong all such errors as result from a failure of two minds to meet, whether the minds of two persons, or the minds of one person at different moments of his history. Should a surveyor, commissioned to measure the S.W. angle of a plot, proceed, through some misreading of his memoranda, to measure the S.E. angle instead, no superior accuracy of measurement would better his chances of discovering his mistake. Nor has science any other way than the ways of common prudence to guard against or discover such mistakes; both must trust to careful checking and rechecking, to reduce to negligible proportions the probability that such a mistake has been made in any given case. What this probability is, how small it should be to be "negligible"—these are matters science itself is content to leave to the judgment of general experience, rather than determine and define by quantitative (statistical) experiment.

But if there is anything unsatisfactory in the recognition that errors of this class can neither be eliminated nor their probability measured and the limits of the negligible defined, there is nothing in this condition to distinguish empirical

from formal science The two are equally exposed to the danger of mistaking the subject of the response for the subject of the question, due to the failure of responding and questioning minds to meet, a failure no less possible when the two minds are those of one person than when they are the minds of two The most practiced calculator, looking up the logarithm of a given number, will occasionally allow the eye to slip from the column or row proper to that number to an adjacent column or row, the danger of committing this mistake is not diminished by using seven place tables rather than five, and there is no way of reducing the chances that such a mistake has been made in a given calculation save that of checking and rechecking In short, everything that can be said about the first of our two classes of mistaken identities can be said with no less truth of those to which formal science is liable than of those to which empirical science is exposed And yet, the formal scientist sees in this state of affairs nothing to rob the unanswerable questions of his science of meaning, nothing, that is, to deter him from defining the answer to these questions as the limiting conception determined by a sequence of progressively accurate responses

It is not, then, to false subject identifications falling within the first of our two classes of confusions that we can look for any condition making the unanswerable questions of empirical science of less meaning, or indeed of other meaning than the questions of formal science And so, if there are possible errors of identification that would have this effect, errors peculiar to metric and not shared by formal operations, they must belong to the second of our two classes of mistaken identities those the possibility of whose detection depends on the size of the probable error with which the predicate term of a response is affected

Are there errors of this class to which metric operations are, and formal operations are not exposed?

There are indeed such errors, and to guard against them is a matter of concern in every metric determination One source of this kind of error, and the way in which science protects itself against the danger of letting it pass unnoticed, may be shown in an example falling within a class of metric questions seemingly so restricted as to suggest that no principle general to all metric problems could be drawn from so simple, or rather, so simplified an illustration However that may be, the example we have in mind is already before us, the danger of committing an error of this kind is implied in our geodetic surveyor's problem of measuring the angle [BAC]₁, only, for greater verisimilitude, we shall suppose a like angle measurement to be submitted, not to the elaborately painstaking geodetic, but to the less meticulous small-area surveyor

Suppose such a surveyor commissioned to establish certain property lines, involving the measurement of an angle BAC In a surveyor's practice, it will frequently happen that he is required to test the accuracy of a set of angle-measurements made at a considerably earlier date In the interval between the two operations, it may well be that one of the landmarks to which the earlier surveyor sighted will have been displaced, i e , changed in its space-relations to a system of other landmarks taken to have remained immovable Suppose the original marker at B to have moved to a point B', not in the line of vision AB, then the angle B'AC will be no more identical with the original angel BAC than is the location of B' with that of B We ask, under what conditions can the magnitude and moment of occurrence of this displacement be detected? Will the necessity test designed in the last chapter suffice to this end?

Certainly not, there are conditions under which a series of measurements, though affected by a gross systematic error, would nevertheless respond to our necessity test as favorably as though no such error, destructive of its responsiveness, had destroyed its responsiveness The possibility here referred to would be realized if, in dividing the series S into the two subseries S' and S" required for the test, all the measurements of the earlier survey had been assigned to S', all those of the later, to S" Since the displacement which accounts for the systematic error affecting S, falls in the interval between the taking of S' and the taking of S", it will affect the standard deviation of neither subseries, and, consequently, the ratio of the two will lie as close to unity as it would have lain, had no displacement of a landmark introduced the systematic error in question Nor need the fractioning of the set S into two subseries in such wise as to let the set respond favorably to our test be restricted to the unique case in which S' comprises all measurements of the earlier, S", all measurements of the later survey One can imagine cases in which, for a limited magnitude of displacement, a limited number of measurements of either of the former subsets could be transferred to the other, and the standard deviations of the resultant subsets be too little disturbed to force the ratio of one to the other to fall without the range to which our necessity-condition restricts it

The foregoing comment would be of little interest, did it not introduce a general principle bearing on the problem of finding a condition whose fulfilment by a set of measurements is sufficient to assure its responsiveness We see, of course, that no one test of a set's conformity with our necessity-condition, applied to a single fractioning of a set into subsets would, if favorably responded to, be sufficient

evidence of the set's responsiveness   But our brief discussion
has said enough to make it clear that, as the number of such
tests applied to different fractionings of a given set increases,
the size of a systematic error (here, introduced by a dis-
placement) that would escape notice decreases   But we also
see that, however large the value of n, so long as it remains
finite, the number of different fractionings of S remains
finite, and that, therefore, for a given value of n, the
maximum number of applications of the necessity-test
would not suffice to exclude the possibility of a systematic
error of a magnitude less than some given magnitude
passing undetected

But the preceding two methods of applying the necessity-
test to a set of measurements are not the only ones at our
disposal, and it suggests itself that before going further with
the discussion of developing a sufficiency-test out of the
application and reapplication of our necessity-test, we stop to
make exhaustive classification of the possibilities open to us

Consider anew the necessity-test formulated in the last
chapter   it required that a set of measurements S be allowed
to fall into the two subsets S' and S", of which the standard
deviations would be respectively σ' and σ''.   Then, the
formula pronounced the set to be irresponsive unless σ'/σ''
lay within the range $(1 \pm f(k,n))$ If now we classify in
terms of the values assignable to n and k, all possible ways
in which this test could be applied to a set of measurements,
we find them to fall under four heads   (i) n and k both
constant, (ii) n constant, k variant, (iii) k constant, n
variant, (iv) k and n both variant

Of these four possibilities, we have examined the first
two, with the following findings   (i) for a given value of n
and at least one value of k, a set might respond favorably to
our necessity-test, yet be affected by a gross systematic

error, (ii) for a given value of n and all possible values of k, a set might respond favorably to a series of tests, each based on a different value of k, yet be affected by a systematic error which, though minimal, was still finite We are then limited to possibilities (iii) and (iv), if we would develop, out of an application and reapplication of our necessity-test, a sufficiency-test, i e, a test that would exclude the possibility of a set being affected by an error though less than any given error

(iii) Letting our thought follow the third way of applying the necessity-test to a set of measurements, we may begin with the assumption that, within limits, any conclusion at which we may arrive will be dependent on the initial value which we assigned to n and k To postpone to a later moment a consideration of these "limits", as well as to give an empirical beginning to a process whose continuation must be left to our imagining, let us return to the set of angle-measurements taken by our surveyor

The 100 measurements comprising this set will prove to be safely above the limit which theory and practice will place upon the lowest value to be assigned to n Now as this "basic" series is lengthened by successive addition of single measurements, we may conceive the lengthening series to fall into a sequence of "supplementary" series, numbering successively 101, 2, 3   measurements each To this basic series and to each of the subsequent supplementary series, our necessity-test is to be applied, choosing for the value of k a number as nearly as possible equal to 2 (i e , for even values of n, the two subseries will number n/2 measurements each, for odd values, the subseries will number respectively $(n + 1)/2$ and $(n - 1)/2$ measurements) A later discussion of the limits set on permissible values of k will show $k = 2$ to be well below the upper limit that

theory and practice place upon the value of k. To have in mind as sharp a picture as possible of the procedure that is to furnish the metric scientist with the best evidence available to him of the responsiveness of a series of measurements, let us follow step by step the treatment to which each one of a sequence of series would be subjected as measurement after measurement was added to the basic series of 100 until a series of (say) 200 measurements had been reached. When this process is completed, we should have a sequence of 101 series, consisting of the basic series of 100 measurements, and 100 supplementary series, each comprised of all the measurements of its antecedent series to which it added one measurement of its own contributing.

Now if we supposed each one of this sequence of 101 series to be subjected to our necessity-test, what control should we acquire over the occurrence in any one of them of an undetected error? Evidently, a control far superior to that which we should have acquired had we subjected any one of the sequence of series just described to a single application of the necessity-test; for no series however numerous would be safe from a systematic error, however sizable, due to a changed condition falling in the interval between the taking of the first and the taking of the second subseries into which it had been fractioned for the application of the test. But if, instead of a test applied to all collectively of our 200 measurements, we imagine the test applied to all distributively of the sequence of 101 series gathered in the course of building up to the 200 total, the result would be vastly different. A displacement that remained undetected by a test applied to any one of this sequence would sooner or later be detected by a test applied to a later series of the sequence—sooner, if the displacement were large; later, if it were small. This, for reasons similar

to those which entered into our discussion of possibility (II), there, n being constant and k variable, the error left undetected for any given value of k would be disclosed by test applied to subseries based on other values of k, *since some of the measurements included in the two subseries S′ and S″ would be interchanged with every change of k* Here, a similar change in the constitution of S′ and S″ would be effected, not by the interchange of the measurements assigned to either, but by the redistribution of measures occasioned by the addition of new measurements, with the result that the constitution of the S′ and the S″ into which a given series, S, of the measurement sequence of 101 series had fallen, would be changed with every step of the process of lengthening and testing each series of the sequence Nor is that all, it has already been shown that the range within which the ratio σ′/σ″ must be confined, if the *series of measurements from which they are calculated is to be a responsive set*, is expressed in a form $(1 \pm f(k,n))$ convergent for a constant value of k and increasing values of n We see, then, that not only *will an error of finite magnitude affecting any one series of our sequence be sooner or later revealed by the failure of a subsequent series to meet the necessity-test*, but also that *the size of an error which would remain undetected at the end of a finite number of n measurements is convergent with increasing values of that number* That is to say, a series of n measurements that meets the requirement whose fulfilment is a necessary condition to *constituting* that series a responsive set, approaches, with increasing values of n, the *limiting conception* of a set from which the possibility of its being affected by a systematic error, though less than any given error, is excluded [1]

(IV) Before considering how the formulation of this

responsiveness-test may advance us toward our final objective, the possibility of defining an answer to the unanswerable questions of metric science, we must return to a matter which a preceding paragraph postponed for later discussion In the course of that paragraph it was said that "within limits" the values assigned to n and k made no difference to the outcome of what we now accept as a responsiveness-test

What are these limits and what conditions set them? The conditions fall into two classes, the first, purely mathematical, the second, partly empirical Limits set by the mathematical structure of the necessity-test are definite but unimportant it is evident that no values could be assigned to k and n which would divide the set S into the two subsets, S' and S", of which one or the other included but a single measurement Limits set, partly by theoretical, partly by practical considerations, are important but indefinite The "practical considerations" carry us back to a warning previously noted a theory whose premises are offered, as are those of least-square-theory, as holding only for an indefinitely numerous set of data, cannot be supposed to hold rigorously for a definitely limited number of data If, then, the theorems which theory would support as furnishing a test of sufficient responsiveness are to be used on sets to which the premises of the theory do not pretend these theorems to be applicable, it must be left to the practical scientific sense of the metricist to decide how numerous his measurements must be to warrant him in accepting any finite set as sufficiently long to make its favorable response to a responsiveness-test a sufficiently safe assumption The graph (p 10 b) of our surveyor's frequency-distribution shows how close a set of 100 measurements can come to meeting the demands imposed by least-square-theory on an

"indefinitely numerous" series, and the progressively
rigorous responsiveness-test just designed would seem to be
as sound a numerical replacement of a frequency-distribu-
tion graph as any that could be deduced from the premises
of least-square-theory alone As for the choice of 2 for a
value of k, its advantage lies in the result that the number of
measurements falling in each of the subseries S' and S'' of
a given series, S, of measurements, is equal and therefore
approaches as close as the value of n allows to meeting the
condition of being "indefinitely numerous"

At the conclusion of the preceding argument, we take
the metric scientist to have joined the formalist in perform-
ing a function essential to either, if he would arrive at a
scientifically supported response to the kind of unanswer
able question addressed to him Each will now have deduced
from the premises of his science the conditions to have met
which is necessary and sufficient evidence that the method
by which he has arrived at a range $(m \pm p)$ warrants him in
returning a response of the form $R(m \pm p)$ to the question
asked It was our failure to formulate a condition not only
necessary but sufficient to provide the metric scientist with
such evidence that blocked our progress at the end of the
last chapter Now, having made good this deficiency, we
return to and follow further the line of thought on which
we there entered

The problem under discussion in those concluding para-
graphs is one that has faced us since we first opened the
door of the metric scientist's workshop to find, if possible,
a meaning for the correlatives, question and answer, when
the question was one of fact-unanswerable as are all
questions addressed to the metric scientist Our plan of
approach has been from the beginning at once simple and
promising, its starting point, the reflection that unanswer-

able questions are to be met with in formal as well as in empirical science, in the former science, the answer to such questions, though not to be found, is to be defined in a way acceptable to all formalists, if then every proposition used by the formalist in defining the answer to a formal unanswerable is also available to the experimentalist in defining an answer to an empirical unanswerable, do not the correlatives, *question* and *answer* have as well-defined a meaning in empirical as in formal science?

In returning to the topic under discussion, we have the same motives for returning to the vocabulary devised for its conduct as we then had for devising it, namely, to deduce from analogous propositions holding respectively for formal and empirical questions and responses, an identical proposition holding for formal and empirical sciences alike That vocabulary gave special meanings to the terms *operation, permissible values, necessity-test* for which last we may now substitute the term *responsiveness-test* The first two terms we may suppose to have been sufficiently defined at the moment of their introduction The last, we now define to be a test such that a series of operations meeting it will have fulfilled the conditions necessary and sufficient to constitute the series a responsive set

With this understanding of terms, we ask ourselves whether the following propositions do not hold for a response returned to either of two questions, the one, asking the number whose square is 2, the other, the number of degrees in the angle [BAC]$_1$

(i) A range of permissible values $(m \pm p)$ established by a responsive set of v operations is qualified to serve as predicate of a response of the form $R(m \pm p)$

(ii) With increasing values of v, (a) the range $(m \pm p)$

established by a set of operations responsive for all values of ν is convergent, and (b) a response of the form $R(m \pm p)$ approaches the limiting conception of a response of the form $R(m \pm 0)$, and (c) any difference which might distinguish the subject of a response from the subject of the question asked is either constantly zero (in formal science) or is convergent (in empirical science) (iii) The coupling verb or a response ("must be taken etc ") is identical in responses of the form $R(m \pm p)$, whether returned to a question of formal or to one of empirical science

Facing these propositions, we ask ourselves whether the argument which pointed to the significance of proposing them does not force the acceptance of their truth If so, does not their truth imply the solution of our underlying problem? Shall we not say The answer to either the formal or the empirical science is a declarative sentence ("The value sought is m") dictated by the limiting conception of an imperative sentence ("The value sought must be taken to be within the range $(m \pm 0)$")? Finally, if this is so, must we not conclude that the correlatives, *question* and *answer*, have no less, indeed no other meaning for the unanswerables of empirical than for those of formal science?

Perhaps, however, in accepting this conclusion as final, we should be going too fast A matter of doubt, suggested at the very beginning of our inquiry, remains unresolved But let the presentation and discussion of this doubt be the topic of a new chapter, while the remainder of this one serve as a manner of footnote to the discussion that has gone before Such a footnote is needed, for in our willingness to advance as uninterruptedly as might be to the conclusion of its argument, we have brushed aside certain matters calling

for comment If we would form a clear picture of the relation of the mathematical outcome of this argument to the practical problems of the experimental scientist, two matters in particular call for attention one, concerning the limitation, the other, concerning the possible generalization of the responsiveness test we have accepted

As has been said, the adequacy of this responsiveness test to warrant the acceptance as a datum for induction of a response $R(m \pm p)$ based on a finite series of measurements, is established as a deduction from the premises of least-square theory Such acceptance is of course for the moment only, a lengthening of the series of measurements from which it is calculated may well discover the presence of a systematic error affecting the series, so that our assurance goes no further than the confidence that if such an error remain undetected at the end of a shorter series, it is bound to be revealed at the end of a sufficiently longer series But, though to this extent theoretically acceptable, our responsiveness test makes no pretence to being practically available for extensive laboratory use One has only to reflect that the "operation" which would raise an approximation of the νth order to one of the $(\nu + 1)$th order would require the gathering of a series of measurements a hundredfold as numerous as those establishing the approximation of lower order to see that the labor of observation and calculation involved in raising approximations of lower to those of higher order would soon transcend the limits of human accomplishment To arrive at measurements available for use as data for induction the laboratorius is driven to look elsewhere for a responsiveness-test fitted to his needs

Nor is the need of economizing labor the only motive that impels the experimental scientist to the search for a responsiveness-test more available for laboratory use than

the one here formulated More serious is the reflection that among the objects which the metricist must submit to observation in the process of "measuring all that is measurable" many more are than are not of such ephemeral nature that only very limited series of measurements could be gathered from them Indeed, a more ultimate reflection reminds us that nowhere in Nature, neither among the everlasting hills nor the sempeternal stars, are objects to be found on which series of measurements of a number greater than any given number could be made

These reflections would make the practical job of carrying out the Galilean program of measurement a hopeless one for the experimental scientist, were he restricted, as we are by the limited scope of our discussion of special problems, to only such responsiveness-testing as can be deduced from the premises of least-square-theory Fortunately for our hopes of "getting somewhere" in the matter of gathering measurements and formulating inductions from them, the laboratorius is not so limited From the premises of techniques other than least-square-theory, he is able to devise a responsiveness test far more serviceable than the one here proposed Into the nature of these techniques, we do not attempt to enquire in the present study, not only for lack of room, but also because statistical scientists who make this problem their special study are far from being agreed among themselves It is enough for us to note that the responsiveness-test we have accepted is conceptually susceptible of experimental application, leaving it to the study of statistical scientists to resolve the problem of alternative tests into a theory less "fluid" than any they have so far been able to agree on

Our second footnote is free from technicalities, and merely calls attention to matters of general knowledge — matters

which the rapid march of our argument toward its main objective has allowed to pass unnoticed, but which it is well to have in mind if we are to form some conception of the range and complexity of the metric operations that must conform to our responsiveness-requirement if they are to furnish measurements from which induction may set out Thus, though the present chapter opened with an example of an error resulting from a mistaken identity due to the displacement of a land mark, the argument which followed created of the type of error generally known as "systematic " A systematic error is to be looked for wherever a longer or shorter sequence of measurements departs from the average of the set to which it belongs, in the same direction and approximately the same amount The displacement pictured in our illustration would of course show the measurements taken in the first survey systematically to depart from the average of the whole set in one direction and to approximately one amount, those taken in the second survey, in the opposite direction and in about a constant amount Here, then, we have an example of a systematic error which the surveyor will ultimately discover to have been the result of a displacement of landmark, but the condition that accounts for this error is but a special case of a broad class of conditions, any one of which could have introduced a systematic error into a series of measurements The other species of this genus is comprised of errors conditioned by the change not of the coordinates individuating the object measured, but of an attribute of that object other than the one to be measured on whose magnitude, however, the magnitude of the measured attribute depends The dependence of length on temperature is a case in point, a mercury thermometer, an example of it known to all the world But what is not known to all

the world is the number of attributes, other than the one to be measured, on whose magnitude the magnitude of this attribute depends. An example which though of fairly recent date has already become a classic of ingenuity and refinement, is furnished by Michelson's determination of the wavelength of red calcium light in terms of the standard meter. A later redetermination of this wavelength is recorded as follows:

> The primary standard of wavelength of light adopted by 3d International Union for Cooperation in Solar Research, as the result of the work of Benoit, Fabry and Perot was as follows: The wavelength of red ray from Cadmium, produced by a tube with electrodes is 6438, 4968 A°[A° = 10⁻⁸ cm.] in *dry air* at 15°C on the hydrogen thermometer, at a *barometric pressure* of 760 mm, the value of *gravity-constant* being 980.67 (latitude 45.) This number will be the definition of the unit of wavelength.[2]

In this report the decimal order of the last digit with which the measured wavelength stops indicates the "permissible error" of the experimental result, while a change in any one of the conditions under which the determination was made (*dry air, temperature, barometric pressure, gravity constant at given latitude*) would have resulted in a change in the value of the required wavelength. With this illustration before us, it will be seen in what sense we called our surveyor's problem "as simple an example as could be found" of a set of measurements affected by a systematic error. This problem owed no peculiar simplicity to the fact the error was the result of a change in coordinates of position rather than of thermometric, barometric, or other

ing a set of measurements susceptible of responding favorably to quite numerous reapplications of the responsiveness-test is not so fortuitous as this argument would make them seem The matter of gathering a responsive set is not entirely a matter of finding, it is, in ever increasing measure as science progresses, a matter of *making* as well The failure of a finite series of measurements to meet the responsiveness-test does indeed convict the set of being irresponsive, but it does not therewith condemn the series to rejection as a whole or even in certain indicated parts It is to be noted that any application of the test which convicts a series of being affected by systematic error, not only detects the presence of an error, but indicates any sequence of measurements subject to the error It might suggest itself that we should stand a better chance of having a more responsive, though less numerous, series of measurements, if we simply discarded those items shown to be erroneous, and retained the remaining measurements as a promising beginning of a series to which new measurements might be added with new hope of success in arriving at a more numerous responsive set Such, indeed, would be the procedure prescribed for the treatment of a sporadically aberrant measurement which Chauvenet's criterion had estimated to be the result of a mistaken instrument-reading or other "accidental" condition

No such expurgation by waste has been science's way of treating systematically aberrant measurements Instead, it has been the insistent and increasingly successful study of science to correlate systematic departure from normal of an attribute to be measured with change of coordinates of an attributes whose measurement is not demanded To show the systematic change in the one to be the result of, and its magnitude to be a function of a measurable change

condition with whose variations a measured quantity might vary Its simplicity consisted in the uniqueness in kind and number of changing conditions that occasioned the error noted, whereas the general case of detecting and correcting for systematic errors would have to provide for errors arising from all kinds of change of condition which would result in a change of the attribute measured

The complexity of the process of experimental measurement to which this generalization of the problem called attention, suggests the need of final footnote This footnote is indeed no more than a comment on its predecessor From the analysis here offered of the conditions to be met by a progressively responsive set of measurements, it would be easy to draw a discouraging inference Does not the analysis imply that the scientist's chance of finding so much as a limited set of measurements meeting the responsiveness-test, is purely a matter of luck, a luck that would have to be indefinitely sustained, if a lengthening set were to continue indefinitely to meet the ever more rigorous test which conditions progressive responsiveness? But how small are the chances of gathering a set of measurements which, if they withstood one application of the responsiveness-test, would continue to withstand fifty or a hundred, let alone an unlimited number of reapplications! And as our study has advanced, such favorable chances as may have seemed at first realizable, have markedly dwindled All that was said in an earlier paragraph pointing to the ephemeral existence of most of the objects whose attributes are to be measured, has since been extended to apply to the transient stability of all individuating coordinates and all classifying attributes of which change in the value of one conditions a change in the value of another

In fact, however, the metric scientist's chances of gather-

ing a set of measurements susceptible of responding favorably to quite numerous reapplications of the responsiveness-test is not so fortuitous as this argument would make them seem The matter of gathering a responsive set is not entirely a matter of finding, it is, in ever increasing measure as science progresses, a matter of *making* as well The failure of a finite series of measurements to meet the responsiveness-test does indeed convict the set of being irresponsive, but it does not therewith condemn the series to rejection as a whole or even in certain indicated parts It is to be noted that any application of the test which convicts a series of being affected by systematic error, not only detects the presence of an error, but indicates any sequence of measurements subject to the error It might suggest itself that we should stand a better chance of having a more responsive, though less numerous, series of measurements, if we simply discarded those items shown to be erroneous, and retained the remaining measurements as a promising beginning of a series to which new measurements might be added with new hope of success in arriving at a more numerous responsive set Such, indeed, would be the procedure prescribed for the treatment of a sporadically aberrant measurement which Chauvenet's criterion had estimated to be the result of a mistaken instrument reading or other "accidental" condition

No such expurgation by waste has been science's way of treating systematically aberrant measurements Instead, it has been the insistent and increasingly successful study of a science to correlate systematic departure from normal of an attribute to be measured with change of coordinates or attributes whose measurement is not demanded To show the systematic change in the one to be the result of, and its magnitude to be a function of a measurable change

in the other—this is science's device for transforming irresponsive series of measurements into responsive sets The richer science's equipment of such functions, the less the scientist's need of rejecting either as a whole or in its affected parts a series affected with systematic error, the better his chance of "making" what would otherwise be the waste product of luckless "finding" into the raw material of a profitable response to a question of fact

The appearance of "finding" and "making" in this last sentence carries one's thought back to the moment when these terms were introduced into the present discussion, by way of asking how far experimental responses to questions of fact were the findings, how far the makings of experimental science? The question will appear many times in many contexts in the course of the present study, but the answer to be given it in the present instance has been sufficiently argued to be sufficiently clear If we would give the last word of our thought on the subject a more epigrammatic form, we might, with no more than a twist of the tongue, phrase it in a way at once critical of certain misinterpretations and approving of accustomed wordings We might say The scientist, sufficiently equipped with previously accumulated inductions, *finds*, that he can *make* the ultimate data of his observation (e g instrument-readings) into something that, without his making, these data would not have furnished, namely, a response to a question of fact

With this brief review of the commonplaces of laboratory, observatory and field, suggesting for future reference something of the complexity of the metric scientist's task in measuring individual coordinates and classifying attributes, our thought may turn back to that "perhaps" on

which the argument hung suspended to make room for
these few footnotes

¹Editorial note Singer, of course, is not interested here in the
technical problems of the test he discusses (e g , compounding of errors),
but rather in the conceptual meaning of such tests, assuming they
could be designed

²Glazebrook, *Dictionary of Applied Physics* (1923), IV, 889

12. Answer to Question of Fact: Coupling Verb

CERTAINLY, THE REASONING FOLLOWED IN THE LAST chapter seemed cogent enough to support its suggested conclusion, the conclusion, namely, that all the propositions whose truth was accepted by the formal scientist in defining the answer to an unanswerable question addressed to him, might safely be accepted by the empirical scientist to a like purpose. Why then should we not accept this conclusion as final, what grounds could one have for further hesitation? Evidently, the only thinkable ground would be such as gave room for a doubt as to whether all the propositions whose truth was assumed by the formalist to the end of defining his answer, could be assumed with equal confidence by the empirical scientist in defining *his* answer. Now of all the propositions whose acceptance by both the formalist and the experimentalist was essential to the framing of identical definitions of an answer to their respective unanswerables, none would seem less open to doubt than that which pointed to the identity of the coupling verb in the response returned by either scientist to an unanswerable question addressed to him "The number whose square is 2 *must be taken to lie within the range* 1 5 $\pm$ 0 5," says the arithmetician, "the number of degrees in the angle [BAC]ᵢ *must be taken to lie within the range* $\bar{S}$ (degrees, minutes, seconds) $\pm$ 0" 1," says the surveyor Are not the coupling verbs in the two responses verbatim identical?

And yet, we must stop to think The coupling verb in the two cases is to be sure verbally identical, but, then, in both

cases it is a verb of command, and (as was pointed out in the
beginning) the compelling force of a command varies with
the authority of the voice that issues it All we can yet say
with considered evidence is that, if any of the three propo-
sitions which hold for and give meaning to the unanswer-
ables of formal science does not hold and give analogous
meaning to the unanswerables of empirical science, it can
only be the third A previous chapter recognized the absolute
authority over the formal scientist of the *must* appearing in
his response, does the *must* of the empirical scientist's re-
sponse exercise a like compulsion on him?

What gives initial point to our question, is the difference
to be noted between the consequences of disregarding the
injunction of formal and of empirical science, respectively
To propose for the real value of $\sqrt{2}$, a number lying outside
the range $(1\ 5 \pm 0\ 5)$ is to advance a formal impossibility,
to propose for the real value of $[BAC]_i$ an angle lying outside
the range $(\bar{S} \pm 0\ ''1)$ is to advance, not a formal impossi-
bility, but an empirical improbability We know the formal
science that establishes this arithmetical impossibility, and
recognize the self-contradiction implicit in a professor of this
science accepting as possible a value which his science
shows to be impossible Does the theory of probabilities
whose postulates our surveyor accepts establish with like
rigor the empirical improbability of values that the sur-
veyor's response puts out of bounds?

For the greater ease of our discussion, it is time we drew
into one picture, or rather into one person, those figures that
we have so far kept artificially contrasted the formal and
the empirical scientist This we may do quite readily, if we
accept as general premise a statement of which the fuller
meaning and justification will develop in the sequel,
namely, that the experimental scientist is himself obliged

to accept the postulates and theorems of a definite system of formal sciences, to which he must "adjust" his observational data, if from them he is to derive any response to a question of fact This system will include, we anticipate, a logic, an arithmetic, a geometry, a kinematics, a mechanics, and, among others, a probability-theory With this understanding, we may transfer at once from the formal to the empirical scientist all responsibility for keeping his outgivings consistent with the proposals of those postulate-sets he has now accepted as his own premises Thus, if our surveyor has accepted the Euclidean as that formal geometry to which he is to adjust his readings, measurements, and configurations, he could not pretend to have found a given triangle to be (say) an isosceles right triangle of 1 km side, unless he could show it consistent with his measurements to attribute to the hypothenuse a length lying within the range $(1\,415 \pm 0\,005)$ km Now suppose that in order to effect this adjustment of his data to a formal geometry, he had been obliged to attribute to at least one of the sides of his triangle a length lying outside the range $(m \pm p)$ km , calculated from his measurements of this side by applying to them the "theory of least squares " He would of course be doing what the *must* of an empirical response enjoins him from doing, thereby defying whatever authority it may be that imposes this *must* upon him, but would he be falling into a self contradiction no less formal than the one he would have committed had he attributed Non-Euclidean proportions to the sides of his adjusted triangle?

If our formal probability theory established $(m \pm p)$ as the range within which were to be found all values whose probability of being the real value of the quantity measured was *greater than zero*, then of course to take for real a value lying outside this range would be to advance an impossi-

bility of the same order as that implied in proposing for the
real value of $\sqrt{2}$ a number lying outside the range
(1 415 $\pm$ 0 005) But the theory of least squares[1] establishes
neither this nor any other finite range within which all
values of a probability *greater than zero* are confined

Or, if our theory established (m $\pm$ p) as the range within
which were to be found all values whose probability of
being real was *sufficiently high* to warrant taking any one of
them for the real value sought, then the self-contradiction
involved in taking for sufficiently probable a value that
one's own accepted premises show not to be sufficiently
probable would be formal enough But our probability
theory does not establish this or any other range of *suffi-
ciently probable* values In fact, no theorem of any formal
science to which our experimental scientist has undertaken
to adjust his findings is contradicted by disregarding the
must of the empirical response, no authority supports it save
the convention of a certain community of scientists to whom
the experimenter wishes to address his response and have it
understood But could not this community at any time re-
vise its convention and restate its standard of sufficient
probability? For, what distinguishes the range (m $\pm$ p) from
any other range of the form (m $\pm$ λ), x being other than p?
It is that (m $\pm$ p) is the range within which one half the
measurements[2] of a responsive set may be expected to fall,
So much our theory of probability establishes as a theorem,
but no theorem of that theory establishes inclusion in this
range as a condition imposed on any value of a measured
quantity whose probability of being the real value of that
quantity is sufficiently high to warrant its acceptance as the
real value of the quantity measured Not until a community
of scientists has agreed on (what we might call) a "com-
munity house rule" to that effect, "must" a member of that

community take the real value of the quantity measured to lie within this range. Are we not then forced to admit that a scientist's obligation to observe the rules of a given club, rather than to attach himself to some other club, or just to "flock by himself," implies in its breach no such self-stultification as would his refusal to remain consistent with the theorems of a formal science (e.g., arithmetic) whose postulates he had accepted as his own premises? And does not this reflection point to a fatal break in those analogies between the propositions that hold for and give meaning to the unanswerable questions of formal, and those that hold true for the like questions of empirical science? The formal and empirical responses have indeed the same coupling verb, but the command that verb expresses does not issue from the same source in the two cases and seems not to be implemented with comparable sanctions.

But even if we accepted this conclusion as destructive of all analogy between the compulsion of any attainable response to formal and to empirical unanswerables, respectively, is it equally fatal to analogy between the limiting conception of the unattainable answer-compelling response to questions of one and those of the other kind? That it is not, follows from theorems of the theory of least squares, which there has so far been no occasion to consider. This theory not only recognizes the mean of a responsive set of measurements to be the most probable value of the quantity measured, but calculates from these measurements just how great this probability is;[2] i.e. assigns to it a place in the probability-scale of fractions lying between 0 (= "practical impossibility") and 1 (= "practical certainty"). Again, the theory shows that as the order of the range (m ± p) converges toward zero, the probability of this most probable value being the real value sought approaches unity as a

limit, i e , its probability approaches practical certainty
Finally, the theory shows that as, with increase in the
number of "operations" comprising a responsive set, the
range $(m \pm p)$ converges, so does every range $(m \pm x)$
analogously defined (i e defined in terms of the percentage
of measurements that may be expected to fall within the
range) We see then that however dependent on the con-
ventions of a community may be the value its members
assign to the x of a range $(m \pm x)$ calculated from a finite
set of measurements, the limiting conception of a range
$(m \pm 0)$ defining the single value practically certain to be
real, is identical for all conventions, whatever their several
choices of x may be Shall we not then conclude that the
compulsion put upon the scientist by the *must* of his answer-
dictating response is the same, whether the question to be
answered concerns the value of $\sqrt{2}$, or the size of the angle
$[BAC]_1$, i e whether the question put is an unanswerable of
formal or of empirical science?

Still, a scruple may remain, born of the thought that,
while the answer conceived to be dictated by the unattain-
able, limiting response of empirical science is identical for
every choice of permissible range, yet every attainable
response is dependent on a choice of range that must be
made, and can be made, only by convention Whereas, the
like response of formal science is the limiting conception of
a sequence of progressively accurate responses in no one of
which the predicate-range of permissible values is in any
way subject to choice Are two dictates equally compulsory,
of which one is merely invariant with the variation of a
choice that may be variously made while the other is deter-
mined by conditions that leave nothing open to choice?

The scruple is perhaps exaggerated, yet its allaying
affords opportunity to make explicit certain assumptions

that have so far been left tacit, and which, like most things left tacit, may crop up to trouble us in later contexts. The general lesson of these explications when made may be forecast from a single illustration of their effect. They will show our arithmetician's response, "$\sqrt{2}$ must be taken to lie within the range (1.415 ± 0.005)" to be no more independent of convention than our surveyor's response, "[BAC]ᵢ must be taken to lie within the range ($\bar{S}$ ± 0″.1)."

To see why this must be, one has only to re-enter any one of those workrooms in which are made the various tools experimental science needs to carry on its work of "measuring all things measurable." For, no one of these tools, these formal sciences, which, from Euclid on, careful hands have fashioned and painstaking experimenters have put to use, is the only one of its kind to which the experimenter could have looked for his instrumental equipment. When their several offerings are ready for use these formal scientists will have expressed their postulates and theorems in terms of quantitative symbols of various kinds and dimensions; the arithmetician, in number symbols; the geometer, in length and angle symbols; other scientists, in symbols of such various dimensions as those distinguishing one or another of the several kinds of quantity appearing in any system of "weights and measures." For these symbols, your experimental scientist must substitute such terms of corresponding dimension as those in which he records his readings, makes his calculations, communicates his responses. These will include numbers that are numerals of some particular number system; lengths and angles that are multiples of some particular length and angle unit; and the like of the rest. Let us bring all these particular number systems, metric scales, and whatever other dimensioned quantities we may have occasion to consider under the general head of

standards Among the standards of any experimenter's set, must be included the criterion of *sufficient probability* that is to determine his range of "permissible values"

Now, any standard of given kind or dimension of which a scientist may make use, is one of an infinite number of the same kind or dimension, of which he might have made just as effective use to the end of recording, computing, communicating his findings No postulate of the formal science in which quantities of this dimension appear, restricts the experimenter's choice to one rather than another of this infinity of possible standards So far from it, that a postulate would lack the generality required of it as premise of a formal science, did it not hold as well for one as for another of an infinity of differently standardized quantities of the same dimensions What is required of these postulates, and all that is required of them, is to show that whatever is expressed in terms of one choice of standards determines what is to be expressed in terms of any other As though to say, formal science imposes no one of an endless number of possible idioms on the experimental scientist accepting its postulates, but it does require that these idioms be intertranslatable

To return now to our arithmetician and our surveyor the *must* of their respective responses exercises on them precisely the same kind of compulsion and accords them the same kind of freedom The arithmetician, having accepted the postulates of a certain arithmetic, cannot without self-contradiction return any other response to the question concerning $\sqrt{2}$ than the one attributed to him, *if* the idiom in which he expresses himself is standardized to the convention of our current decimal system The surveyor, having accepted the postulates of least square-theory, cannot return any other than the answer quoted to the

question concerning the angle [BAC], *if* his chosen idiom
is standardized to the convention limiting sufficiently pro-
bable values to the range (m ± p) But no self contradic-
tion is implicit in another arithmetician, accepting the
same postulates but speaking in terms of an octesimal or
duodecimal number system, returning a quite different
response, and no contradiction is implicit in the return by
another surveyor, accepting the same probability-theory,
but speaking in terms of a sufficient probability standardized
to a range (m ± x), in which x is other than p, of a response
other than that returned by the surveyor of our *Report*

To complete these reflections on the *musts* of science,
one question remains to be considered Although no postu-
late of science imposes upon the scientist the use of one
rather than another of several intertranslatable idioms, yet
one may wonder whether there may not be some other
requirement implicit in the meaning of experimental
evidence which, when made explicit, would uniquely
determine the *right* choice of standards What suggests the
possibility of an affirmative answer, is the simple fact of
history, that of the infinity of standards available for
recording data and communicating results, very few of any
given kind or dimension have had historic use, or even
advocacy To let the case of number systems illustrate what
would be found equally true of any other of the underlying
standards of measurement, one finds, in all but the most
primitive stages of culture, systems having for radix some
multiple of 5 to have had by far the most extensive use Of
these our present decimal system is the sole civilized sur-
vivor, while in the way of alternative radices, 8 (Archi-
medes) and 12 (the "duodecimalists") are the only ones that
have had distinguished, if as yet ineffective,[4] advocacy
What influence has restricted historic preference to so few

out of an infinity of possible number systems? Whatever that influence, may it not in the course of scientific development find acceptance, not merely as a restraining control, but as a uniquely determining requirement?

Now, as to the first question, the generally accepted answer recognizes the motive influencing choice to have been one of "convenience" It is not hard to see the convenience of a number system based on a multiple of 5 to those earlier culture-stages in which the arithmetician was largely dependent on an abacus for the working out of his sums, the convenience, namely, of having an abacus always at hand Or again, in the matter of length-measurement, one sees a little convenience in having attached to one's person such measuring-rods as would let him "scale off" an inch, span, cubit, yard To be sure, the game of "convenience" is not always so easily played It would take an expert philologist to trace to their origins the various special conveniences served by the various unit-weight and measures that have appeared in the course of history, but it is safe to assume that, where data are not lacking, their convenience could be shown

No doubt this explanation of choice in terms of convenience is satisfactory enough, but the better the evidence of the convenience served by any historic choice of standard, the clearer the evidence that every convenience achieved has been won at the price of some inconvenience incurred, and that, what price may at one time have seemed worth paying, will generally have come, at a later moment of cultural progress, to be deemed excessive For example, however important it may have been to the primitive computer to have had a digital abacus always at hand, to an arithmetic that has long forgotten the use of an abacus, a certain inconvenience attendant on the use of a decimal

system may well seem too high a price to pay for a convenience that great as it was for the dead, is none for the living The disadvantage under which the user of a decimal, rather than (say) a duodecimal system suffers, lies of course in the more limited factorability of the decimal radix 10 having for factors only 2 and 5, 12, on the other hand, 2, 3, 4, 6 In recent times the advantage of this greater factorability has seemed to a not inconsiderable number of "duodecimalists" sufficient reason for a progressive community immediately discarding the decimal idiom, in which it has for so long been accustomed to speak, in favor of a duodecimal which it could acquire at very little cost to present parents, at no cost to their future offspring But even should the arguments of the duodecimalists prevail, and the change in number system be made, there is nothing to show that the duodecimal system would remain the idiom of mankind for all ages to come Neither here nor elsewhere in the array of standards can one see the history of repeated change ending in a future of changeless standards, definitely assured of having attained the greatest possible convenience

Risking, then, a safe enough *ab uno disce omnes*, the general lesson we would draw from the example of number and length-standards is this The imperative verb coupling the subject and predicate of responses to unanswerable questions exercises the same compulsion and implies the same exemption from control, whether the question be one of formal or of empirical science

To sum up the case for identical compulsion, reflection on scientific method finds us always in presence of two co operating scientists having as their common acceptance a standardized idiom of the kind an experimenter must use in recording his data and communicating his responses We may picture the two as (1) a formal scientist constructing

and offering, and (2) an experimental scientist accepting and adjusting his findings to one or another system of deductive sciences The formal scientist's work is done, if so difficult a task could ever be finally done, when he has shown a given set of postulates and theorems to constitute a perfect deductive system The experimenter's work begins with a search among these deductive systems for a set of them (e g a logic, arithmetic, geometry,   probability theory) to which he finds that he can adjust his data in a way to permit his responding to such questions of fact as are submitted to him

With this, we have completed the task of comparing the propositions that hold for the responses of formal and of empirical science, respectively In either science, addressed with an unanswerable question, the same differences distinguish the predicate, subject and coupling verb of a response in the form R (m ± p) from the like elements of an answer in the form A(n) In spite of these differences, not to be eliminated from any attainable response, the fact that such responses were susceptible of being formed with "progressive accuracy" allowed formal science to define, what it could never find, the answer to an unanswerable question Since all the propositions that, holding for a formal response, make such defining possible, seem to have their analogues in empirical science, shall we not conclude that the same possibility of defining the answer to an unanswerable question is open to empirical (at least, metric) science also?

The discovery in the next chapter that we may not thus conclude, that we cannot give this meaning to the unanswerables of metric science, must reopen the whole question posed by our "cautious mind " If the unanswerable

questions of metric science cannot have the meaning
just discussed, then what, if any, meaning can they have?
It can only be hoped that the disillusionment forcing us to
reopen this difficult question will be repaid by such enlightenment as to the whole scope and meaning of experimental
evidence as to requite us for the labor of attaining to it.

[1] Our discussion must for the present confine itself to the classic
theory, accepting, without being able altogether to interpret, the out
lying portions of the Gaussian curve The generalizations of classic
theory, originating in the work of Pearson and continuing to develop,
must be left for another context [But this has not been done, see
remarks in Introduction —Ed]

[2] Editorial Note At this point there appears to be some technical
inaccuracy in the text As defined on p 106, $(m \pm p)$ is the range in
which one half of the means of samples of size n would lie—assuming a
sufficiently large overall set of readings has been obtained But the
meaning of the text remains the same regardless of this point

[3] Editorial Note That is, the mean calculated to the last significant
decimal place Least squares theory does not, of course, imply a positive
probability to any specific real number But the calculated mean is
itself a range of numbers

[4] Editorial Note reflecting the fact that this was written in the age
before computers!

13. Propositions and Postulates: The Real and Ideal

THE DECEPTION INTO WHICH THE ARGUMENT OF THE last chapter might well have led us, lies in nothing there said, but in something left unsaid To repeat the statement on which all the rest of the argument depends for its promise of furnishing a definition of *fact*, the permissible error of an approximation based on ν operations is convergent, and the mean of the set approaches a limit with increasing values of ν With this truth, we see fulfilled a necessary condition for according to an unanswerable question of empirical science, the same clear meaning that is universally accorded to a like question of formal science

But is the fulfilment of this necessary condition also sufficient to validate such an understanding of question and answer? We might too readily take it to be, arguing that as the number of operations yielding an approximation of the νth order could be increased at will, it would always be possible, by sufficiently extending this number, to reduce the error sufficiently to raise any approximation of the νth order to be one of the $(\nu + 1)$th order And this would be true, if we could be sure that for all values of ν a method of operating which at the end of ν operations had proven responsive would continue to prove responsive at the end of a number of operations sufficiently greater than ν to have raised the resultant approximation from the νth to the $(\nu + 1)$th order But have we any such assurance? Certainly, there is nothing in the one proposition that has been advanced as holding for both formal and empirical operations to imply this conclusion, there is nothing to exclude the

possibility that a method of operating, proven responsive for ν operations, will prove irresponsive for some or all numbers greater than ν. Yet, until this possibility is definitely excluded, for at least one method of operating, responsive to a given single-valued question, there is no ground for according to that question a meaning of the second of our three classes of meaningful questions. We know the question to be unanswerable, but we do not know it to be susceptible of progressively accurate response; or, as we may now put it, susceptible of progressive approximation. How should we be able to acquire such knowledge? To this question there can be but one answer: the method by which the formal scientist establishes the required property for his response to a question concerning $\sqrt{2}$ is the only one by which the empirical scientist could establish it for his question concerning the size of [BAC]$_\nu$. To show an unanswerable question to be susceptible of endlessly progressive approximation, the problem of both scientists is the same: it is, to find a responsive method of operating such that if the method yield an approximation of νth order, it can be made to yield an approximation of the $(\nu + 1)$th order, for all values of ν.

If we turn first to the formal and then to the empirical scientist, to ask whether his science is able to solve this problem, the answers they give, so far from continuing to be analogous are completely contradictory. For the formal scientist, it is formally necessary that any method of calculation which had furnished an approximation of the νth order could be made to yield one of the $(\nu + 1)$th order, *for all values of* ν. For the empirical scientist, it is practically certain that no method of measurement which has furnished an approximation of the νth order can be made to yield one of the $(\nu + 1)$th order, *for all values of* ν.

As the reason for the empirical scientist's assurance in making so sweeping a statement must occupy us extensively in the sequel, let us here consider only the consequence it must have for the hopes entertained at the end of the last chapter It is of course fatal to them, the supposed analogy on which they were based turns into a complete contrast In the matter of single-valued questions, your mathematician knows an infinity of them to be answerable, another infinity to be unanswerable yet susceptible of progressive approximation Your metric scientist knows none to be answerable, and *knows* of none that is susceptible of progressive approximation But does the experimental scientist know any question of this class to be *not* susceptible of such approximation? The issue may seem to turn on a distinction too subtle to give importance to the way it is answered, the distinction, namely, between *not knowing* of any metric question that *is*, and *knowing of* at least one that it is *not* susceptible of progressive approximation Yet, on this issue depends, not only the outcome of our immediate enquiry into the meaning of fact, but, in the remote sequel, the issue between two of the most important schools of "natural philosophy" that history has to tell of

Whatever may be our final conclusion in this matter, we must recognize at the outset that nothing so far said would put the metric question beyond possibility of progressive approximation The experimental scientist has indeed set it down as a practical certainty that no *one* method of measurement can carry his response to such questions beyond a finite order of approximation He has asserted, on what ground remains to be seen, that there will always be an upper limit to the number of measurements taken by any one method that can be made to constitute a responsive set of measurements After this number has been reached, the

method by which they were taken will be found to "break down," i.e., to prove irresponsive. And since only a responsive set of measurements can yield an approximation of any order, the possibility of raising an order of approximation attained by any given method, breaks down with the method. Nor is this the only consequence of a breakdown in the method by which a given approximation has been attained. It is a corollary of the theory of probability on which our test of responsiveness was based, that a method of measurement, proven irresponsive for *any*, is proven irresponsive for *all* values of v. What the breakdown has shown is no less than this: that, unless and until science shall have found some other method of measuring the quantity asked about, it has no experimental ground for returning any response to the question asked. When one recalls that the scientist has already asserted it to be a matter of practical certainty that no method of measurement could ever be devised that would not break down for some finite value of v, one sees the seriousness of the situation facing the reflective mind seeking to make explicit the meaning of *fact* implicit in the words and works of the experimental scientist. There is but one course for reflection to pursue at this point, and that is, to persist in the plan followed from the beginning; we must continue to look over the shoulder of the historic scientist at work, to watch carefully what he has done when, faced with a breakdown in method, he has been left without experimental authority to return any response to a question of fact put to him.

The general answer we know: the whole history of scientific progress is nothing but the story of repeated reactions to the question, What is to be done when a method of measurement, hitherto accepted as responsive, breaks down? Wherever science has counted itself successful in meeting a

situation of this kind, it has simply changed its method of measurement For the discredited method, it has substituted an amended procedure, capable of yielding an approximation of higher order than any attained by the discredited method, without itself breaking down

With these matters in mind, we may begin to feel that the break in the bonds of analogy between propositions holding for the unanswerable questions of formal and of empirical science, respectively, instead of hampering, may ultimately ease our way to an understanding of fact It does indeed prevent us from defining the real fact as the limiting conception of a goal to be approached by prolonging indefinitely a known method of operating, but may it not relieve us of the need of so doing? So far as we can yet see, there is nothing to limit the possibility of science continuing, throughout an endless future, the performance of its past. And that performance has been, to advance the order of an approximation, not exclusively, nor even principally, by increasing the number of measurements taken by a single method, but by inventing new method after new method in such wise that with each new invention the order of approximation obtained will have been advanced at least one point, with no breakdown yet in evidence

If we were assured that nothing in the course of experience or in the way of reflection could arise to block this endless progress, the understanding that such progress would require a repeated change of method, not merely repetition of an operation prescribed by a single method of measurement, might lead us to modify, without forcing us to abandon the interpretation of *fact* as a limiting conception But have we assurance of the unlimited possibility of progressive approximation effected by experimental method?

Experimental scientists would, one thinks, be at one in denying that evidence now in hand furnished any such assurance, but not at one in their reasons for making this denial There are three opinions on the subject (1) Within quite recent years, some close laboratory observers have come to think that in certain kinds of measurement we had already touched that last decimal place which no new methods or new instruments could make into a penultimate Knowing how, in matters of measurement, "all things are fast knit together," one might well infer from this that the same check awaited us at a decimal place not yet reached in every kind of measurement (2) But for far longer years and in vastly greater numbers, other thinkers have maintained that our most common and familiar experience of life held ample evidence to the effect that all Nature is subject to an indeterminateness such that no experimental determination of a magnitude could ever be, or come indefinitely close to being, exact Finally, (3) most if not all of those who remain unconvinced by the arguments supporting either of these opinions, are themselves unprepared to accept any evidence now in hand as sufficient to exclude the possibility of future science establishing a finite limit to the order or approximation attainable by experimental method

Of course, the arguments of the indeterminists, whether those who think we already have, or those who think we ultimately must come upon convincing evidence that there is an upper limit to ν, at which an approximation to the νth order cannot be raised to one of the $(\nu +1)$th order, must be given careful consideration But before these arguments shall have been examined, weighed, and accepted as conclusive, we are in the position of agnostics, unable to build on either alternative premise we may neither assume it

proven nor assume it disproven, that the possibility of rais-
ing an approximation of the vth to one of the $(v + 1)$th
order is independent of the value of v Till then, what
meaning, if any, can we attach to the correlatives *question
of fact* and *answer thereto*?

How approach a problem of such baffling difficulty? Yet
it is a problem no study of experimental evidence, its consti-
tution, cogency, possible scope of application, can escape
What understanding could we hope to have of a scientific
method, if we could attach no meaning to the key words
which enter into the discourse of that science? That *question
of fact* is such a key work in the discussion of experimental
method is amply shown by the part it plays in setting the
tasks and following the acts of laboratory-, observatory-, and
field-worker, but how central is its position among the
terms in constant scientific use and long historic interest, is
only to be judged when we shall have seen how dependent
for its meaning is each of these terms on the meaning to be
accorded a question of fact—a matter to which we shall
return later

Meanwhile, we have at least established this much   the
difficulty attendant on defining question of fact is not, as
some have supposed, due to the conditions that make all
such questions unanswerable, an infinity of questions
addressed to the formal scientist are similarly unanswer-
able, yet the answer which cannot be found may in all these
cases be well defined, and with it, its correlative, the ques-
tion, made meaningful   But then, too, *we have seen why the
method by which the formal scientist defines the answer to
his unanswerable questions is not available to the empirical
scientist for defining an answer to his questions of fact*   The
formal scientist is able to deduce from the premises of his
science, the conclusion that no approximation to the

required answer would be an approximation of any order, unless it were assured that the operation by which an approximation of that order had been obtained could by being continued yield an approximation of the next higher order The empirical scientist has not, nor ever can have, any such assurance On the other hand, so long as he has no greater assurance to the contrary, it is, we think, meaningful for him to regard the answer to his unanswerable question well defined if given the meaning it would have for one who "accepted" a certain postulate What this postulate is, what it means to "accept a postulate", why the answer to a question of fact is definable by one, and only by one, who accepts the postulate in question—these are matters for close thinking

The term *postulate* in its modern use is viewed with misgiving by one bred in the discipline of experimental science The "modern use" here referred to, was introduced by Kant, in his study of the problem of the "good life " Here, too, the question to be decided had to do with the possibility ⁿf an endless progress toward an unattainable but inefinitely approachable goal "For a rational but finite eing," Kant had written, "the only possible life is an ndless progress from lower to higher degrees of moral erfection "¹ But then he had convinced himself that this fe was possible for a finite being only if three conditions ere fulfilled These conditions were presented and discussed under the headings God, freedom and immortality ut whether or not these conditions were fulfilled in the orld of realities, was for Kant a question no amount of nite experience, no depth of finite reflection, could ever iscover So far as human knowledge was able to reach, the uestion of their fulfilment must remain forever open pon which, Kant decides that, since a rational man's only

chance of "happy" existence depends on devoting himself to the "endless *struggle from lower to higher degrees of moral perfection*," he can do no better than live *as though* these conditions were fulfilled. He must *postulate* their fulfilment.

To most men who would count themselves rational, however finite, it will seem far from reasonable to act on the assumption that a certain proposition is true, on no better ground than the conviction that it can never be proven untrue, and that one will be the happier for assuming it to be true. Few "rational" atheists will have been converted to theism by the appeal of Pascal's wager: To bet on God, for to win is to win everything, to lose is to lose nothing.

But if the scientist has good reason for eyeing with suspicion the part played by *postulating in the chapters of Kant's moral philosophy*, he cannot have the same reason for being critical of the role to be assigned it in our further thoughts on questions of fact. To set forth these thoughts as succinctly and consecutively as possible, let us start at the beginning.

The premises from which the whole argument sets out, are far from being postulates in any sense; they are simple propositions making no claim on our acceptance save as they find support in experience and reflection. These premises may be listed *as follows*:

Propositions holding for both formal questions concerning the value of an irrational number, and empirical questions concerning the magnitude of a measurable attribute or *coordinate*: Neither question is answerable; neither is meaningful unless the answer which cannot be found can be defined. Neither *answer is to be defined save as the propo-*sition dictated by an answer-dictating response of the form

R(m $\pm$ 0) In neither case is a response of the form R(m $\pm$ 0) to be defined save as the limiting form approached by a series of responses R(m $\pm$ p)$_\nu$, in which the value of p is convergent for increasing values of ν.

Propositions holding for A Formal unanswerables A method of operating is always to be found which, at the end of ν operations yields a response of the form R(m $\pm$ p), such that for increasing values of ν, p$_\nu$ is convergent and m$_\nu$ approaches a mathematical limit

In light of this proposition, the correlatives *question* and *answer* are considered by the formalist to have taken on well-defined meaning

B Empirical unanswerables A method of operating is sometimes to be found which at the end of ν operations yields a response of the form R (m $\pm$ p)$_\nu$ such that for a finite increase in the value of ν, p$_\nu$ converges, but no one method of operating is to be found yielding a response of the form R (m $\pm$ p)$_\nu$ convergent for all values of ν

When a method of operating has "broken down" after some finite number of operations, a revised method of operating is sometimes found, yielding a series of responses of the form R(m $\pm$ p)$_\nu$ in which p$_\nu$ converges to a higher value of ν

At this point in our thinking, in view of our previous understanding that no evidence will ever be forthcoming to prove the process of replacement described in B to be always successfully effected, the last word on the meaning to be accorded the correlatives empirical *question of fact* and *answer thereto* can be given no other form than that implied

in the following General Postulate A succession of operational methods is always *to be sought*, such that each successive method shall yield a response of the form R(m ± p)ᵥ for which the value of *v* is higher than that of any response yielded by its predecessor

Only for one who accepts this postulate as the regulative principle of his scientific labors can a question of fact have a meaning For him, it has the meaning of what in accordance with common use we may provisionally call an *ideal* The postulate is evidently no proposition to be accepted as true or rejected as false, it is explicitly a *proposal* to be acceded to if agreeable to one's plan of life, declined, if obnoxious How inclusive is the plan to the pursuit of which acquiescence in this proposal is an essential condition, is a matter form to be considered only after long preliminary preparation For the moment, we say no more than has already been said only for one so ultimately motivated that the pursuit of the ideal just defined is essential to the pursuit of his ultimate purpose, has an empirical question of fact a well defined meaning

The fact thus defined as "ideal," is remote enough from the "hard fact" of which the "realist" supposes himself to be in secure possession, and on which he pretends to base his rule of conduct But if the idealist's conception of fact is far removed from the realist's, his conceptions of *truth* and *reality* are no less so, for on the foregoing definition of fact, the definitions of *truth* and *reality* are *entirely dependent* for their meaning Consider, the term *truth* In all the world there is but one thing that can be either true or false, and that is a proposition Now, in all the foregoing discussion of matters of fact, there is but one moment at which the term *proposition* enters, the moment which recognized the term *proposition* as distinguishing the form of sentence

in which the answer to a question of fact must be worded
But so far as our thought has gone, a proposition, to be
accepted as answer to such a question, must be one dictated
by the limiting conception of an answer-dictating response
of the form $R(m \pm 0)$ Evidently then the term *truth* can
take on meaning for experimental science only if the
limiting conception of an answer-dictating response can be
given a meaning, and this limiting conception can take on
meaning only so far as no evidence is forthcoming to estab-
lish an upper limit on the value of ν at which an approxi-
mation of the νth order can be raised to one of the $(\nu + 1)$th
order

What meaning our thought has been able to suggest for
truth determines the meaning to be accorded *reality* As the
adjective *true* applies only to a proposition as a whole, so the
adjective *real* applies only to the predicate of a true proposi-
tion Would one know what is meant by the real value of
the angle [BAC], surely it is the value which would appear
in the predicate $(m \pm 0)$ of an answer dictating response of
the form $R(m \pm 0)$ Unless then the term *truth*, a property
common to all true propositions, have a meaning, neither
can the term *reality*, a property common to the predicates of
all true propositions

The hold on *fact* with which our reflections have left us,
is tenuous, indeed as compared with a Gradgrind's firm
grasp of that one reliable item of obtainable information,
and the idealist's conception of a true statement of fact as
an unattainable goal affecting present conduct only as it
inspires one who accepts that goal as his own to bend every
effort to advancing step by step toward it, is remote enough
from the realist's understanding that such statements are
the only safe footholds from which the searcher for knowl-
edge can step off Yet, our understanding of science as a

step by step progress toward the unattainable end of fact-, truth-, reality-finding, seems not at all remote from the understanding of science entertained by the active workman in the experimenter's workshop Such science, writes De Sitter, "is always a struggle for the last decimal place, and the great triumphs of science are gained when, by new methods and new instruments, the last decimal place is made into the penultimate "[2] Our own understanding of the day by day work of the scientist *is no different from this*, and if an observatory-man, who so sees an endless task before his kind, is justly to be called an idealist, he has surely some claim to be called as realistic as anyone can be, an apparent *contradictio in adjecto* only to be resolved by recognizing that the "real" of experimental science is an "ideal "

But if we cannot be alarmed by the tenuous nature of the experimentalist's hold on *fact, truth, reality*, we may be more disturbed by a reflection which makes our grasp of these essentials even more precarious De Sitter's words do not tell us his attitude toward the scientist's prospect of success or failure in his eternal struggle to make "the last decimal place into the penultimate", but our own position on the matter in has been clearly set forth We recognize that no evidence can ever be forthcoming to assure the scientist's unlimited ability to progress, in the sense just defined But we cannot share in the Kantian postulate's assurance, that final evidence of failure is equally beyond our "finite" reach To exclude the possibility of establishing evidence conclusively proving that we must come to a value of ν such that no approximation of the νth can be raised to the $(\nu + 1)$th order, is to reject without examination, not to say without refutation, the arguments adduced by the "indeterminists" of whatever type, establishing a finite

limit to the possibility of progressive approximation If these
arguments should, on final examination, prove convincing,
the result must disappoint the hopes of the present study,
but for that very reason the possibility of that disappoint-
ment must be squarely faced from the moment it is recog-
nized And squarely faced, the completeness of our defeat is
all too easily stated it would mean that the only meaning
the present study is able to suggest as defining the correla-
tives *question of fact* and *answer thereto* is unavailable, and
as we have no other suggestion to make, the outcome would
be much the same as that faced—and so often faced—by the
scientist who at the end of a laborious experiment finds its
result "negative " He can claim no other profit of his
labors than such as lies in having cleared the deck for some
happier experimenter who will have devised a more con-
clusive test of an idea This much, however, we must insist
on no understanding of experimental method which leaves
the meaning of *fact* undefined, can possibly be final, it
would be an "understanding" that left everything worth
understanding to be understood

But, however uncertain our outlook, this very uncertainty
does design the course our further study must take it must
be so designed as to give our conception of *fact, truth, reality*
the best chance of not proving a failure As already an-
nounced, our examination into this critical matter will
continue in the course our study has followed from the
beginning that of watching the experimental scientist at
work But here a certain question has become more impor-
tant than it has been in the past *what* experimental
scientist, or *which* experimental scientists shall we watch?
As to the "what," our choice is easily stated no scientist can
serve the purpose of our study save one whose name, date,
and particular contribution to progress are not of record in

the pages of history But as to which of the many figures
that people the pages of history are to be selected for our
study, nothing more can be said at the outset than to set
down a few general principles controlling our choice First,
*each character will have met and overcome an obstacle to
progress* that at the moment of his appearance offered the
most obstinate resistance to "making a last decimal place
into a penultimate " But among historic contributors to the
progress of science, it is easier to find one who does than one
who does not directly or indirectly meet this requirement
What we evidently face is a problem of sampling employing
a special technique of which too much cannot be said at the
outset, no more in fact than a suggestion of the motives
guiding our choice To make the enterprise manageable,
we shall keep our samples so far as possible within the class
of scientists history groups under a *single name,* for the
reason that the problems they face, however varied, have
something in common—e g , *astronomers* But to be able
to tell some part of the history of astronomy—say the
period from Copernicus to the present day—and to tell it in
such a way that the story shall have a profitable *haec fabula
docet* for its moral, this makes a major demand on our
sampling technique Our study of history would be most
profitable if at its conclusion we should have succeeded in
effecting such an exhaustive classification of the various
devices by which obstacles to progress had been overcome,
as to make the classes among which these devices are to be
distributed mutually *exclusive and collectively exhaustive*
of the methods by which any experimental science could
overcome any obstacle of like kind *To be sure, the expres-
sion "of like kind" leaves much to be explained what
conditions do these historic problems of astronomy have in
common not shared by other obstacles to progress with*

which other scientists may have to contend? On this subject
we need say no more at the moment, than to explain that
for convenience we shall restrict ourselves [for the present]
to a review of science' triumph over only such obstacles as do
not involve the difficulties to which the indeterminists
point as presenting insurmountable obstacles to an un-
limited conversion of last decimal places into penultimate
These difficulties will be reserved for [later] study, where
we shall have had opportunity for making new beginnings
without interrupting the continuity of thought followed on
beginnings already made For the rest, the success and
worthwhileness of our sampling technique must be left to
the judgment of the critic as the story of astronomy un-
folds itself The plan just outlined will at least recommend
itself as offering the best defense consistent with thorough-
ness that our study could devise against that final defeat
which we have admitted must be the fate of our attempt to
find a meaning for questions of fact, should it be con-
vincingly shown that the possibility of making last decimal
places into penultimate must meet with a permanent check
at some finite decimal place

[1]K d p V
[2]W de Sitter, *Kosmos*, Harvard Univ Press, 1932, p 134

14. Fact of Science and Fact of Nature: of Fact and Law

WHEN ONE COMES TO LOOK BACK ON THE PERIOD OF scientific development the immediately following chapters are to recall, it may well be that he will experience a certain wonder at what he had previously taken to be a perfectly simple matter Wonder, namely, at the price science has had to pay to come by an insight into what is really meant by, and what justifies, a sentence generally accepted as little more than a tautology "Every fact of experience is a fact of Nature" That this should seem so obvious to common understanding, is the result of our general willingness to think of Nature as the sum of facts independently found, of which each would remain just what it is in itself, whatever the context in which it might happen to be found To the scientist, however, this simple sentence means something quite different, something so far from obvious that it has taken him *centuries to see*, and then only imperfectly, the depth of its implication, namely, this That every fact of experience is a fact of Nature, just because science can approach a knowledge of the simplest fact only as it approaches a knowledge of the total Nature of which that fact is a fragment The full meaning and import of the difference thus briefly hinted at will, one thinks, reveal itself *as our study of a certain scientific development progresses*

The limited sample of history whose critical moments these chapters are to recall, covers the development of astronomy from Copernicus to Bessel, a period of less than three centuries The chapters will indeed recall, rather than

reproduce in any documented way, the episodes of this history. That is, they will imagine an astronomer of our day to reconstruct, for demonstration purposes, the most economical series of observations that would, under stated conditions, have justified an observer of that date in judging a given method of measurement responsive to a given question of fact; then, with like economy, show how a lengthened series of measurements so taken, would reveal an irresponsiveness that not only puts a check to closer approximation, but invalidates such "approximation" as had previously been accepted. To recall how the astronomer has met a succession of such "critical moments" in the history of his science, should furnish us with a limited but significant sample of the resources experimental science has at its disposal for the invention of new methods of measurement wherever new observations have revealed the irresponsiveness of old methods.

To follow this historic development in the most convenient way, it will be well to change the question of fact asked, from one concerning the size of an angle to one asking the length of a line. In place of our surveyor's problem, to measure the size of a certain terrestrial angle [BAC], we will set our astronomer to finding the length of a certain celestial distance, [SM], where S and M are the centers of Sun and Mars respectively, and t_i the moment of time for which this distance is required. The difference in method involved in this transition from angle to length measurement is, in this particular case, less marked than it would have been, had the ultimate readings to be taken changed from angle-readings to length-readings. But all the readings by which an astronomer would arrive at measurements of the distance $[SM]_i$, or any other celestial distance, must remain angle readings; it being manifestly impossible to

"step off" with tape or chain the celestial intervals with which astronomy is concerned From angle readings alone, however, it is quite possible for the astronomer to arrive at measurements of such distances in terms of Earth's diameter as unit length What multiple of the length of a certain metal rod in the *Archives*, Paris, or, more ultimately, of the wavelength of red-cadmium light, Earth's diameter may be, does not concern our present discussion

For the first modern impersonation of an historic figure to whom we shall submit this question as to the length $[SM]_i$, let us choose one who assumes the attitude of an astronomer of about A D 1600, that is, one who will have accepted the Copernican theory of planetary orbits As a Copernican, he will suppose Mars to revolve in a sun-centered circle, with uniform angular velocity The problem *he sets himself will be, to determine the radius of* $[SM]$ of this circle To this end, we will suppose him making a series of measurements, as closely as possible in the manner of those made by Tycho Brahe, which later served as data for Kepler's calculations

We need not reconstruct the technical process by which the astronomer, taking the required angle readings and substituting them in classic Euclidean formulae, would arrive at a single measurement of the distance $[SM]_i$ at a moment t_i We will simply take it for assured that our astronomer has a means of making λ such measurements, at moments t_1 t_2 t_k, so closely clustered in time that, whatever the path Mars may describe around the Sun, the arc completed in the brief interval t_1 t_k cannot have departed so markedly from the arc of a circle as to let the astronomer *notice* anything irresponsive in the set of measurements l_1 l_2 l_k, treated exactly as our surveyor treated the 100 measurements of the angle $[BAC]_i$

He would, then, be entirely justified in taking the arith-
metical mean of the k measurements to be the most
probable value of the distance $[SM]_1$, which we shall write
L_1, at the moment t_1, when the first measurement was
made, letting the probable error of this mean set the limits
of the range $(L \pm p)_1$ within which $[SM]_1$ must be taken
to be In this manner our astronomer, following in the
footsteps of Brahe will establish the values L_1 L_2 L_7
for 7 consecutive oppositions of Mars These oppositions,
let us note for future reference, being separated by intervals
of two years and forty-nine days would find Mars in 7 quite
different relations to the other bodies of the solar system

Now suppose our astronomer to have tried to compose
these 7 sets of k measurements each, into a set of $7k$
measurements, exactly as our surveyor might have success-
fully composed his 100 angle measurements of 10 subsets of
10 measurements each, or as the astronomer himself might
have composed the k length measurements of each distance
L_1, of n subsets of k/n measurements each Would he have
succeeded, i e would he have found the series of $7k$
measurements to constitute a responsive set?

The post-Keplerian astronomer knows that he would not,
and he knows what change from the method of measure-
ment followed by the surveyor, or by our imagined astrono-
mer himself in finding the values L_1 L_2 L_7, Kepler
was obliged to make before he could compose the data of his
readings into a set of measurements responsive to a question
as to the distance $[SM]$ But more important to the ordering
of our thought on changes of method and their possible
variety than the change Kepler made, is the part of previous
procedure that he did not change, the part that no one on
this or any other occasion could have changed Its principle
is changeless It is moreover so simple and obvious that

every scientist takes it for granted, which is no doubt the reason why not only the layman, but the whole tradition of Empiricist philosophy has overlooked it

It is this Whatever the magnitude to be measured, any approximation to its vaue at a moment t_i must depend for its data on a series of measurements made at a succession of moments t_1 t_2 t_n, of which no one need be, and no more than one can be, the moment t_i for which the value is demanded That a measurement made at a moment other than that for which a value is required should serve as any kind of a datum for approximating that value, is a meaningful proposition only on the premise that the value of the magnitude at the moment of measurement determines its value at the other moment for which its value is sought The assumption is, of course, that the measured value *determines*, not that it *equals* the required value, though the possibility of this special case is not excluded Mathematically speaking, the determining condition need be supposed to hold only between the moment for which the approximation is required, and each of the n discrete moments at which measurements are made In all the history we shall have to consider, the relation taken to hold for these discrete moments either is, or is corollary of a conditioning equation assumed to hold as between any two moments t_a and $t_ω$ of a finite time interval If to this interval initial and final limits are to be set, consideration of these limits does not fall within the range of our present reconstruction

These simple considerations put us in position to formulate a general statement of the problem set the scientist who would arrive at an approximation to the distance between two points (here S, M) at a moment t, given a series of measurements of this distance made at a succession of moments t_1 t_2 t_n

Let $l_j \left.\right]_{j=n}^{j=1}$ represent the series of measurements made

at the moments $t_1\, t_2\, \ldots\, t_n$; and let $l_{ij} \left.\right]_{j=n}^{j=1}$ be the values

of l_i at the moment t_i as determined by the equation

$$l_{ij} = f\,(l_j,\, t_j,\, t_i).$$

Then the problem set the scientist is to find a form of the function, f, such that the n values $l_{i1}\, l_{i2}\, \ldots\, l_{in}$ shall form a responsive set of measurements. This found, the mean (L_i) of the set of n measurements l_{ij} will be the most probable value of the distance $[SM]_i$; the probable error of that mean (p_i) will set the limits of the range $(L_i \pm p_i)$ within which $[SM]_i$ must be taken to lie, and therewith determines the order of approximation at which this metric operation will have arrived.

Now, it will be seen that the transition from the Copernican to the Keplerian image of the fractional solar system, Sun, Mars, consists solely in a change in the form accorded the function f. The purely geometrical function (into which *time* does not enter as a variable), by which angle readings are composed into the single length measurement l_{ij}, are identical in the two calculations. What, in light of the observations of Tycho Brahe, Kepler could not leave unchanged, was the Copernican form of the function f; for to try, in the manner Copernicus, to determine the values l_{ij} by use of a function f that makes l_{ij} *equal* l_i for all values of j, yields a series of measurements that do not form a responsive set. Yet, it was this same method that we have supposed our Keplerian reconstruction of events to have used in arriving at seven most probable values of $[SM]$ at seven successive oppositions of M. For this limited number of close-clustered measurements, the Copernican method of

measurement yielded responsive sets and therewith approximations of definite order It was only when the number of measurements taken by this method was increased, and the conditions under which the measurements were taken correspondingly varied, that the Copernican method "broke down" By changing from the Copernican to the Keplerian equation for determining the values l_v from the measurements l_j, the series of measurements resulting again constituted a responsive set, reestablished an approximation of determinate order to the required distance $[SM]_i$, and seemed available for continued progress to approximations of still higher order

But if the foregoing analysis correctly interprets the experimental scientist's understanding of the mutual dependence of progressive approximation to fact, and progressive approach to law, it is equally remote from the current layman's and the traditional Empiricist's understanding of the relation between empirical fact finding and law seeking How far from the latter, is witnessed by the painstaking analysis J S Mill offers in his *Logic* of this same Keplerian moment in the history of astronomy

"The object of Kepler was to determine the real path described by each of the planets, or let us say by the planet Mars (since it was of that body that he first established the two of his three laws which did not require a comparison of planets) To do this there was no other mode than that of direct observation, and all which observation could do was to ascertain a great number of the successive places of the planet, or rather, of its apparent places That the planet occupied successively all these positions, or at all events, positions which produced the same impressions on the eye,

and that it passed from one of these to another insensibly, and without any apparent breach of continuity; thus much the senses, with the aid of the proper instruments, could ascertain. What Kepler did more than this, was to find what sort of a curve these different points would make, supposing them to be all joined together. He expressed the whole series of the observed places of Mars by what Dr. Whewell calls the general conception of an ellipse."[1]

With this picture of "fitting sought laws to known facts," the traditional Empiricist's and current popular understanding of experimental method stop. Not so the scientist's: his account of that method neither does nor can stop here. A moment's reflection uncovers the self-contradiction into which science would fall, did it suppose the Keplerian procedure as described by Mill to have made final or even consistent use of the observational data at its disposal. Mill's account of this procedure will appear consistent only to one who supposes the "direct observation", by which the positions of S and M, and therewith the distances $L_1 L_2 \ldots L_7$, have been established, to have made use of no assumption as to the laws of orbital motion which might (or might not) later be found to fit these "facts". Our modern astronomer's reconstruction of the method by which Kepler arrived at his initial estimates of these distances, made use of a very definite law of orbital motion: *it was the Copernican law of circular orbits*. But this is one of the very laws that Kepler's final results showed to be untrue; i.e. incompatible with the "facts" it had been used to establish. In other words, *some* "facts of the case" have been established by taking for true a law which *more* "facts of the case" show to be untrue.

What, then, is to be the next step the scientist will take to escape this contradiction, to him so obvious, from Mill so hidden? It will be, it can only be, to replace the Copernican form of the function, f, used in (what we may now call) his *exploratory estimates* of the distances L_1, L_2, L_7, by a function whose form is deducible from laws of planetary motion that, since all observations of these distances can be adjusted to them, must be taken to lie closer to the true laws than the Copernican, to which Brahe's observations could not be adjusted

But is even this the end of the story, and may science, having re established its facts consistently with the law these facts are taken to establish, lay aside its problem of planetary motion as solved, or solved at least so far as the observational data in hand can contribute to its solution? Obviously not, nor will the story ever end We are at the beginning of an endless process of fitting laws to facts and facts to laws, in a sense that supports a proposition we may put in the form Every response to a question of fact implies the acceptance of a law, every response to a question of law implies the acceptance of a fact

It will be seen that this conclusion, while it is far from establishing, is at least consistent with the principles of a philosophy that would fill the historically "empty box" of the classification frame constructed at the close of Chapter 7 This "box," it will be remembered, was to hold a theory of evidence whose distinguishing thesis was summed up in the purposely vernacular wording Knowledge of law implies knowledge of fact, knowledge of fact implies knowledge of law As our reflection on the evidence acceptable to experimental science develops, the meaning this colloquial "knowledge of" takes on for science will continue gradually to unfold

The preceding paragraph may be taken to sum up all that our thought can gather to its immediate profit, from reflecting on the historical transition from Copernican to Keplerian astronomy From this first episode of a history of scientific development whose course it has undertaken to follow, our study might be expected to turn to the next chapter of that story But in the way of so direct a procedure, lies a practical difficulty

The transition from Copernicus to Kepler was so simple and straightforward in its motives, methods and results as to allow its story to be told in plain words, slightly helped out at the end by the simplest of algebraic symbolisms Were we, however, to preserve this verbal and algebraic idiom in following the later chapters of this history, we should find the proportion of word and symbol to change the discussion would become less and less verbal, more and more algebraic To devise and follow a symbolism adapted to our purpose, would tax in equal measure the ingenuity of its inventor and the patience of its interpreter This strain on the attention of both is to be avoided, if possible And it is possible For without loss of theoretic rigor and with great relief to our powers of imagination, we may change at this point from an algebraic to a graphic idiom

To do this, one has only to bring into his service that classic organon or tool of scientific thought to which occasional reference has been made in the past under the rather modernized name of "mechanical imagery" But one who takes up this tool for his present ease cannot easily lay it down again until the task it lightens is finished And so, our study, in accepting the help of mechanical imagery, may be expected to need its help for the entire length of its undertaking To employ this tool effectively, one must, of course, have a better understanding of its structure than

was needed for such casual reference to it as was made in previous pages Better, then, devote the remainder of the present chapter to such formal defining of terms and careful explaining of their use as will assure a firm grasp on the meaning and functioning of mechanical imagery

Definitions

1 *A point image of a natural system* is a manifold of points so individuated as to stand in one to one correspondence with the individual points of the natural system observed, and so provided with properties as to make the properties of each point in the image identical with those of its corresponding point in the object

2 *A mechanical schema* is a manifold of points

   (a) individuated by space time coordinates,

   (b) classified in terms of certain structural properties, and

   (c) subject to mathematical conditions such that the values of all variables representing these coordinates and properties being given for any one value of an independent variable, t, are determined for any second value of t

3 The *mechanical image* of a natural system is a point image of that system, conforming to the requirements of a given mechanical schema

Comment

a Point images may be, and historically have been, either mechanical or non mechanical in pattern The earliest sample of consistently mechanical imagery is to be found in the atomic system of Democritus (5th century, B C) The most consequent example of non mechanical imagery is presented by the 17th C monadic system of Leibnitz The difference between the two will be fully explained in the

sequel; for the moment our interest is confined to defining the mechanical point image.

b. The independent variable, t, of Def. 2 (*mechanical schema*), denotes the *time coordinate*; the remaining variables needed to individuate its points, the *space coordinates* of the schema.

c. If t_i is less than t_j, t_i denotes the *earlier*, t_j the *later* of two dates.

d. *Structural*, are such point-properties as are *invariant*; *nonstructural*, such as *vary* with variation of coordinates or properties of other points of a schema.

e. Mechanical images that conform to schemata differing in:

(1) the geometry of their coordinate systems (e.g. Euclidean, Non-Euclidean; n—dimensional, $(n \pm k)$—dimensional), or

(2) their determining laws,

are said to differ in *pattern*.

f. Images of like pattern, differing in the value of the coordinates, or of the properties of the points composing them, are said to differ in *point distribution*.

g. Between the point distribution of dates t_i and t_j, resp., $t_i \leqslant t_j$, a *cause-effect relation* exists.[2]

h. Since the point-distribution of cause and effect respectively are reciprocally determining, each must furnish *all* point conditions needed in an imagery of given pattern to determine the other. A natural system of which an image fulfilling this condition can be formed is said to be a *closed system*.

Such, indeed, as set forth in these definitions and comments, is the conception of mechanical imagery; but to find anything in the world of perception with which the points of a mechanical image might be brought in one-to-one

correspondence, presents difficulties of an order that many find baffling, some, insoluble These difficulties are matters of old and general experience, they are sufficiently serious to have furnished many with grounds for denying to the tool of formal imagery any part in realizing the objectives of empirical science General and ancient as they are, they may nevertheless be best introduced in the form of an example, neither historically ancient, nor scientifically general

Of the modern uses of mechanical imagery in doing the work of experimental science, none is older, more generally accepted, more profitably exploited than one that fills an early page in the history of post-Newtonian astronomy From its beginning, this astronomy accepted as a working hypothesis the assumption that a mechanical image of our solar system could be constructed of only seven points, all individuated by Euclidean spaces and Newtonian time coordinates, each possessed of but two structural properties, mass and velocity, and all subject to the determining law of universal gravitation Given, by way of the point distribution at a date t_i, the coordinates, gravitational masses, and velocities of points imaging the centers of gravity of the sun and of its six planetary systems (planets and their satellites),[3] Newton's Law would determine a point distribution of like description for any second data, t_j The tenability of this hypothesis has from the first been subject to experimental test from the point distribution observed at any earlier date, the Newtonian astronomer could predict the distribution to be expected at a later date Since astronomy continued in the use of this working hypothesis (with only such changes in its description of point distribution as resulted from the discovery of the planets Uranus, Neptune, Pluto), down to a very recent moment, it is to be assumed that the

astronomer found his expectations based on the hypothesis to have been "satisfactorily" fulfilled

But what satisfaction one might find in the empirical confirmation of the hypothesis that our solar system could be brought into one-to-one correspondence with a mechanical image of this sort, may well be disturbed by a reflection of a purely logical order If experience seems to justify, does logic permit this imaging of our solar system as a *closed* system which is the only kind of system a mechanical image can represent?

To admit an affirmative answer, presents a difficulty that seems tantamount to tolerating a self-contradiction For what else than a formal self-contradiction can it be, to suppose the bodies of our solar system to be susceptible of an imagery that can only represent a closed system, when the very determining law of the image is the Newtonian a law requiring every body in the universe to attract every other body in that universe with a force directly proportional to the product of their masses and inversely to the square of the distance between them? How, then, can the scientist's image represent as *closed*, a natural system limited to so inconspicuous a fragment of Nature as the whole company of our sun and its planets adds up to? How can he pretend the vast sidereal world lying without, to have no influence on the motion of bodies lying within this minute fraction of the universe?

The question here raised seemed to be the worry of a very special science, to lack (as was forecast in presenting it) generality of scientific interest Yet, as our study advances, it will confirm what its previous moments may have led us to suspect, that, namely, the paradox into which the Newtonian astronomer seems to have fallen is not peculiar to him or to his science. It is general to every conception of

natural law that would imply the impossibility of any limited part of Nature constituting a permanently closed system. More than that, it is general to every scientific method of responding to questions, whether of fact or law, that would assume objects of experience to conform to any formal condition imposed by any formal science. Yet, how can any experimental method of responding to questions put to science escape the necessity of assuming many such conformities?

Neither the question nor the difficulty of answering it is new to these pages. How, we asked in the past, suppose some limited number of measurements actually made, to be so many random specimens of an indefinite number of measurements not actually made? How is the surveyor to find in experience a triangle whose three measured angles conform to the Euclidean requirement of totaling 180°? How is the chemist to find in his laboratory a compound, the masses of whose constituents conform to the mass-conserving requirement of adding up to the mass of the whole?

Nor are questions of this order new to history. Your modern scientist does but generalize from a classic model when he returns to all these and the like questions the answer Plato gave to some of them. The most refined experiment cannot find in Nature anything of any kind that conforms to any requirement imposed by a formal science on anything of its kind. And yet, as we have seen, to respond to any question put to it, experimental science must proceed *as though* its findings had conformed to conditions imposed by one or more formal sciences.

A simply worded yet adequate solution of paradoxes of this class may well begin with a definition. If, namely, the data obtained from observing a natural system are such as

would have been obtained from observing a system in which certain formal conditions were accurately fulfilled, these data are said to be *adjustable to* an image so conditioned

Thus, the instrument readings from which our surveyor calculated his 100 measurements of the angle BAC, were such as he would have read from his instrument, had he been observing a system in which BAC was mathematically constant Those from which this surveyor calculated the three angles of the triangle BAC, were such as he would have obtained from observing a system in which BAC was a Euclidean plane triangle Finally, those on which were based the Newtonian astronomer's predictions and verifications, were such as would have been obtained from observing a completely closed system In view of these observations, our definition permits us to say that the surveyor and the astronomer arrived at their several results only after having *adjusted* their readings to formal images in which respectively were fulfilled the conditions specified the constancy of a given angle, the Euclidean requirements for the planeness of a given triangle, the physical requirement for the closedness of a natural system

And, if this interpretation of the part played by formal conditions in modern science is consistent with experimental practice, it is not inconsistent with Platonic tradition To have found our observations of a natural system "adjustable" to formal conditions, is not to have "found" in Nature a thing fulfilling these conditions With all adjustments made, the surveyor will not have *found* in Nature a constant angle, or a Euclidean plane triangle, the astronomer will not have *found* there a closed system To have *found* any one of these formally conditioned objects, not only must the finder's observations have been adjusted to the formal images of an object so conditioned, but they must have been

adjustable to no second image of an object otherwise conditioned And that is exactly what the scientist does not claim for his adjusted observations, on the contrary he accepts as general the principle that observational data adjustable to one are adjustable to more than one formal image

Hence, to return to the paradox that most concerns our present study, just as no contradiction would be implied in the position of a scientist who, on whatever grounds, should have accepted the Heraclitean premise that nothing in Nature remains constant for any finite period of time, yet found his observations of a given angle adjustable to a formal image in which the image of that angle was subject to the condition of mathematical constancy, so no contradiction is implied in the Newtonian astronomer's acceptance of the Newtonian Law as a premise from which it followed that no natural system could be a closed system, yet basing his predictions and verifications on a mechanical (and therefore closed) image of the solar system to which he had *adjusted* his observations of that natural system

Considering the difficulties of a logical order that have led some to deny any essential part to formal science in achieving the ends of empirical science to have been effectively resolved, we may examine into the role formal science does play in arriving at experimental results

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[1]*Logic*, Book III, Chapter 11, § 5
[1]To have recognized the cause effect relation to exist between a given distribution and itself, is to have classed cause effect with those relations which the formal logician calls *reflexive* At the present point of our development, there is no obvious reason why we should choose to regard this relation as reflexive rather than non reflexive, existing, e g only between an earlier and later point distribution Indeed, since our classification takes the historical concept, *causa sua*, to be meaningful, we will

seem gratuitously to have accepted responsibility for clinging to an
archaic and highly suspect term But motives for making this choice
will appear later in our study To give them a name without, for the
moment, a definition, the cause effect relation is here taken to be re
flexive in order to keep its formal properties distinct from those of
another relation commonly identified with it, the *producer product*
relation The producer product relation, when defined, will, of course,
be classed as non reflexive

    ²To recall the planets known in Newton's day were, in the order of
their distances from the sun Mercury, Venus, Earth, Mars, Jupiter,
Saturn

15. Postulates of Metric Science: Historic Recognition

THE FIRST USE TO BE MADE OF THIS TOOL OF IMAGERY with which we are now provided, is to formulate, with the ease this instrument affords us, the demands a science must meet, if it is to realize a certain objective, the objective, namely, of responding to a given question of fact To have stated this objective in terms of fact rather than law, is not to have narrowed the scope of the science into whose demands we are to inquire, for what has been shown in a particular case will be found to hold in general responding to questions of fact and responding to questions of law are problems not to be dissociated, progress toward the answering of either implies a progress *pari passu* toward the answering of the other But an experimentally founded approach to a single-valued fact can always be expressed as an approximation of determinate order and equal steps of progress measured by equal elevations of order Whereas approach to a uniquely determined law is not to be so expressed or progress toward it so measured, the narrowing of (what we may call) the range of permissible laws, is only evidenced in the diminishing region of uncertainty surrounding science's estimate of the facts that any permissible law is required to "fit" To "fit," indeed, but to fit in the sense that only such laws can possibly fit the facts as are themselves fit to establish approximations to the facts in question For this reason, the progress commonly called "progress of science" is only to be measured in the terms De Sitter uses for the purpose In these terms, one will recall,

science is said to advance a step whenever "by new methods or new instruments the last decimal place is made into the penultimate." It must nonetheless be understood that such progress toward a single-valued fact implies and indirectly measures progress toward a uniquely determined law.

As an example of a question of fact to which science must always be in need of a response, our discussion has chosen one concerning the distance between two given points in Nature; for example, the centers of Sun and Earth at a given moment of time. But of deeper interest to science than the "determining" of any one such distance, is the establishing of a method by which any such distance may be determined. However important it may have been to the astronomy of the day in which the laws of planetary motion depended on the value a Copernicus, Kepler, Newton found for the distance $[SM]_0$ of greater importance to the science of all days is the method to be employed in any day for the finding of any given distance. Let us, then, lend new depth to our study by considering the objective of science to be, not the determining of some given distance, but the establishment of a method by which any distance may be determined.

Considering this to be the objective science would realize, if it could, we see that its first concern must be, to formulate all requirements whose fulfilment is a necessary condition to the realization of this objective. As these requirements are demands to be met, if a certain objective is to be realized, it is appropriate to call these requirements *postulates*; a term well calculated to remind us that any form of words so designated expresses a proposal to be accepted or declined, not a proposition to be affirmed or denied. Each postulate is the consequent (C) of a hypothetical proposition whose antecedent (A) expresses an objective to be realized; and

while the integral proposition, "if A is to be done, then C is to be done," is certainly true or false, the consequent alone can be neither Standing alone, the consequent is a postulate, or demand, of the kind Kant called "a hypothetical imperative," i e it is the consequent of a proposition of which the antecedent is tacitly presupposed

The discussions of the last chapter lets us set down at once the first of the demands to be met if a distance is to be measured If, namely, the distance at a given moment of time between two points given in Nature is to be determined, then

Postulate 1 Images of the two points are to be included in at least one mechanical image to which have been adjusted all data obtained by observing the natural system in which the points occur

Until the demand of Post 1 had been met, science would be without equations by which the distance between the two points, P_1 and P_2, at the moment t, could be determined from the distance between these points measured at any second moment t_j And did not values $[P_1P_2]$, so determined constitute a responsive set, the ultimate data (here, instrument readings) obtained by observing the natural system in which P_1 and P_2 occurred, would *not have been adjusted* to the mechanical image used in determining these values If, however, the requirement of Post 1 *has been met*, then the responsive set of values $[P_1P_2]$ will have established *such an approximation of determinate order* as could serve as predicate to a response to the question asked And only a method of measurement enabling the scientist so to respond could be a method of determining distances in general

With the fulfilment of Post 1 and consequent establishment of an approximation of determinate order to the

required distance $[P_1 P_2]$, science will have satisfied a condition necessary to responding to the question asked concerning this distance But is the fulfilment of Post 1 not only a necessary but also a sufficient condition to so responding? In previous chapters, we have taken it to be so, but in these chapters, the assumption was tacitly made that the responding scientist was in possession of but one approximation to the distance in question This assumption, having served the passing purpose of canalising toward its sole issue, a discussion of the meaning to be given the correlatives *question* and *answer*, must now be lifted For, from the general principle that data adjustable to one will be adjustable to more than one image of the natural system observed, and from the understanding that each such image is to be used by a method of measurement yielding an approximation, it follows that the scientist will always be faced with more than one approximation to the distance to be determined These necessarily discrepant results may or may not be "significantly different", the degree of difference that is to be counted "significant" is determined by the general theory of measurement Where the scientist is faced with significantly different approximations to the same quantity, no response to a question concerning the quantity approximated to is permissible Where no significant difference separates any two of his approximations, the same general theory of measurement provides the technique by which from his array of "compatible" approximations, a most probable resultant can be calculated

The fulfilment of Post 1 may then have either of two results, the one commanding, the other forbidding response We ask ourselves, with the first result, is the objective of science realized, with the second, is it proven impossible of realization?

Whatever the ultimate answer to this question, we see at once a condition on whose fulfilment affirmations must depend A general method of determining a distance must be independent not only of the individual points that terminate it, but of the *number of measurements from which an approximation to it shall have been calculated* As any approximation must have been made on the basis of some finite number of measurements, the condition of being faced with only compatible or with some incompatible approximations will establish the objective of science as realized or as unrealizable only if that condition remain unchanged with indefinite increase in the number of measurements establishing that condition We see, then, that a science, having successfully met the requirement of Post 1 is faced with a second demand whose fulfilment is no less necessary to the realization of its objective, namely,

Postulate 11 If all observations have been adjusted to one or more mechanical images of the natural system observed, the number of measurements based on each image is to be increased until the order of approximation previously attained by each method so based shall have been raised at least one unit

From what has already been said concerning the theory and practice of metric science, one can readily see the results science may expect from any continued attempt to meet this self-repeating demand First, *with sufficient increase in the number of measurements made by each of two incompatible methods*, one or the other will be the first to break down, and with it, the evidence that all observational data could be adjusted to the image of which that method made use Second, *with sufficient increase in the number of*

measurements made by each of any two compatible methods, successfully satisfying Post 11, the approximations of sufficiently higher order attained by each will prove incompatible For, as the theory of measurement shows, the difference between two approximations that is to be regarded as "significant" is a direct function of their respective probable errors, and as the probable error established by a responsive set of measurements is convergent with increase in the number of measurements constituting the set, so too is the degree of difference taken to be significant

If these two were the only results the scientist need anticipate to follow on the continued attempt to meet Post 11, he would forecast the future of a science set on determining a given distance to be a succession of moments in the course of which it was alternately faced with only compatible and with some incompatible approximations, alternately faced, then, with conditions permitting and conditions forbidding response But there is a third result which he foresees must follow on a sufficient increase in the number of measurements taken by each available method, namely, the breakdown of all available methods And therewith would be discredited the hypothesis that had made these methods available, namely, the hypothesis that they satisfied the requirements of Post 1

What, then, is science to do, when with the breakdown of all previously available methods of measurement, it finds itself unable to fulfil the first condition necessary to realizing, or even approaching a realization of its objective?

No doubt, in this situation, the scientist could, and generally does, have recourse to history, to gather from it what he can find there in the way of devices that have been successfully used in the removing of like obstacles to

the realization of his objective But, proceeding thus gropingly and unmethodically through a maze of literature, how could he ever be sure of having overlooked no device that would have solved his problem, had he happened to stumble upon it? The assurance he craves could be afforded him if, and only if a postulate could be framed, demonstrably exhaustive of all possible devices by which his problem could be solved "All possible devices," for did it provide for but n such devices when (n + l) were possible, the use of one or another of these n devices would not be a necessary condition to the solution of the problem set One sees the scientist's need for an exhaustive classification of "*all possible devices,*" *but is there any thinkable way of meeting this need?*

Whether or not the only grounds the present study has to suggest on which a postulate demanding such a classification could rest for the assurance of its exhaustiveness are sufficiently broad to justify such assurance, is a question only to be considered after these grounds have been examined What reasons there may be for thinking them so, depends upon the fact that the premises from which they are deduced are propositions purporting to be supported by an experience common to all men, not merely by the experimental findings of a limited class of exceptionally trained men

These grounds being so common and commonplace may well be introduced by a homely analogy Among the humbler data of general experience, might be counted the case in which an only available pair of shoes proved a misfit for one's foot, what are the only things to be done about it, if one would not go unshod? Would one not be limited to one of two procedures either *reshaping the shoe to fit the foot,* or *remolding the foot to fit the shoe?*

Suppose now a scientist, bent on approximating the distance between two given points, to have found his observational data unadjustable to any previously available image of the natural system in which the points occurred, would he not be limited to one of two expedients for meeting the demand of *Post 1* either (1) to recast the image to which data were to be adjusted, or (2) to revise the data to be adjusted?

To try the first expedient, (A), he must devise an image differing from any previously available either (a_1) in pattern, or (a_2) in point-distribution To try the second (B) he must revise the data either (b_1) as to the method of their adjustment, or (b_2) as to the data to be adjusted.

If these be accepted as the only possible ways in which science is to escape the ultimate defeat of the purpose attributed to it, a postulate formulating a third condition necessary of fulfilment, if this purpose is to be realized, would be worded as follows

Postulate 111 If of the methods that, for a limited number of measurements, had met the requirement of *Post 1*, none was found to meet that of *Post 11*, then science is to revise *either* (A) the image to which adjustment is to be made,

      (a_1) as to pattern, or

      (a_2) as to point distribution,

or (B) the process of adjustment

      (b_1) as to method of adjusting, or

      (b_2) as to data to be adjusted

But what assurance have we that the assumption which makes this array of alternative devices exhaustive is itself justified? The present study can suggest no test of the validity of this assumption more conclusive than such as is

furnished by the experience of science itself Does a survey
of the experience of a developing science discover the
scientist to have successfully met the threat of defeat conse-
quent on the breakdown of all previously available
methods but not by introducing changes of method falling
under one or another of the heads listed in Post iii? If so,
the postulate does not exhaust all possible resources of
science, the use of one or another of the expedients it dis-
tinguishes is not a necessary condition to the realization of
the scientist's objective, the objective, namely of establish-
ing a general method of determining a distance But if not,
then one can conclude no more than that his best study has
found no evidence of the resources of science in the way of
overcoming obstacles being richer than those common
experiences recognized to be available

Obviously, a conclusion based on such evidence as this
can never be conclusive At the same time, the testing of
our postulates by such evidence as is to be gathered from the
most exhaustive possible survey of history, made or in the
making, is an obligation not to be avoided Nor has the
present study, from the moment of suggesting an examina-
tion of history as a way, however inadequate, of forming
some conception of the resources of science for the over-
coming of obstacles, proposed to avoid it Yet, to meet it, a
review of history whose ultimate purpose lies well beyond
anything to be accomplished by a logico-historical examina-
tion of experimental science in its development, can offer
no more than a microscopic sample of the vast and compli-
cated fabric of historic science It was as a particularly
manageable sample that a brief period in the history of a
very special science was chosen for examination If so much
that is painstaking and tedious lies between a first beginning
and a present resumption of the study of this bit of history,

it is that the present writer could find no more economical way of defining the purpose of providing the apparatus for conducting this or any other logico-historical study of the kind. Having fulfilled this function as well as might be, we may return to the story whose first chapter has been told.

No question but that the change of mechanical imagery introduced by Kepler falls exclusively within the first of the classes listed under Post. iii, i.e. class (a_1). Both the Copernican and the Keplerian images of the natural system, Sun-Mars, consist of but two points, S and M; both, assign to the two points the same relative location and the same structural properties (inertial mass and velocity), at a given moment of time. The two images differ only in their respective laws determining the point-distribution at any second moment t_2, given the distribution at any first moment t_1. The difference between the two laws is of the slightest. Both treat Sun-Mars as a closed system, both conform with the requirements of Kepler's laws of elliptical orbits and conservation of areas. Only, Copernicus takes the orbital ellipse to be a circle; of whose two coincident foci the sun is situated. Kepler takes the orbital ellipse to be a near but not exact circle; at one of whose two non-coincident foci the sun is situated. On these premises, the Copernican radius vector SM could conserve areas only by preserving a constant angular velocity; the Keplerian, only by exhibiting an alternately positive and negative angular acceleration. But slight as was this difference between two determining laws distinguishing two otherwise identical mechanical images, the replacement of the Copernican by the Keplerian method of measurement was a change in pattern of the image to which adjustment was to be made, without which the requirement of Post. i, given the post-Copernican data with which Kepler had been furnished, could not have been met.

Before leaving the Keplerian conception of planetary motion, one may note for the sake of comparison with later conceptions of our solar system, that the completed work of Kepler extended to every other planet of the system the same treatment his first publication accorded the planet Mars Kepler imaged every natural system, S-P, where P is any planet of S, as a closed system, the solar system itself, is a closed system composed of as many constituent closed systems as there were planets revolving about the sun How markedly this conception of the solar system was changed at the end of the next episode our history records, is of major interest to the scientist, in that it illustrates a device, not needed by Kepler, but elsewhere indispensable, by which a brokendown method of measurement may be replaced by another that withstands all tests of responsiveness available at the time of its acceptance

To turn this page of history, with no more said of the Keplerian conception of the solar system, might raise in a documented mind some question as to the adequacy of our account of his completed system It might, namely, be complained that to represent Kepler as imagining the solar system to be a closed system composed of closed, and therefore mutually independent systems, is to overlook the significance of his third law of planetary motions, the *Law of Sesquiplicate Proportion* This law does indeed formulate a mathematical condition that reduces the number of independent parameters affecting the orbital motions of any two planets, P_i and P_j, from 4 to 3 For it establishes the proportion

$$R_i^2 \quad T_i^3 \quad R_j^2 \quad T_j^3$$

where R_i and R_j are respectively, the mean distance of the planets P_i and P_j from S, T_i and T_j respectively, the time of

a complete revolution of P_i and P_j around S. Given this "conditioning equation," the measured values of any 3 of its 4 variables determine the fourth.

But though this third Keplerian Law might be taken to voice a suspicion that planetary motions may not be so mutually independent as the first two laws take them to be, yet this suspicion falls far short of the Newtonian recognition of the "openness" of any system S_i–P_i to the influence of any second system S_j–P_j. In the first place, Kepler's third law is a purely mathematical induction from the independently determined values of R and T for each of the solar planets, on whose orbits it imposes the condition

$$R^2/T^3 = \text{const.}$$

Newton's analogous law of proportion was a deduction from the determining law of that mechanical image to which he was able to adjust certain post-Keplerian observations that could no longer be fitted to the Keplerian image. Then, too, Kepler's induction was only a close approach to, not mathematically identical with Newton's deduction. Kepler's equation could be deduced from Newton's premises only if the masses of the planets were taken to be mathematically zero instead of being, as they are, "negligibly small" as compared with the mass of the sun. With this word of explanation, we may turn to the history of those happenings that necessitated a replacement of Keplerian by some new type of imagery; a necessity that was met by the transition from Kepler to Newton. Our system of postulates requires the astronomer who, so far as available evidence shows, has met the conditions of Post. i, to follow the prescription of Post. ii. He is to increase the number of measurements made by the method used to establish an approximation of the vth order, till he shall have raised it to an approximation of

the $(\nu + 1)$ th order The theory of least squares requires that either a method of measurement must, with a sufficient increase in the number of measurements taken, effect this elevation in an order of approximation previously attained, or be discredited as a responsive method of measurement, i e as a method available for establishing an approximation of *any* order

So, in fact, did the post-Keplerian astronomer proceed he multiplied the number of measurements of [SM] made by the Keplerian method, until he found this method of measurement to break down For, well before Newton's time, these observations revealed the impossibility of adjusting measurements based on the data they furnished by a Keplerian image of the solar system Not that this evidence of a breakdown in the Keplerian method would have been furnished by every addition of new measurements, whatever the dates of their taking For, of the new observations made, those falling within limited periods of time remained adjustable to a Keplerian image of the solar system There were, however, other intervals within which such observations as were taken must have seemed to their first interpreters more closely adjustable to a "bulge" in the Keplerian ellipse than to the ellipse itself, while the velocities as measured for these moments showed a corresponding departure from those required for the Keplerian "conservation of areas " Post-Newtonian astronomy knows what these "bulges" are, and under what conditions they occur The so-called "bulges" are now known as "perturbations", and the intervals in which they are *noticeable* are those in which the planet under observation *is first approaching nearer to,* then *receding further from,* some near-lying planet But not until Newton had developed his conception of our sun and its planets as a gravitational system was science in

possession of a mechanical image to which all post-Keplerian observations could be adjusted.

What then of this new type of image? Does every change in Keplerian imagery introduced by Newton fall within one or the other of the classes which, under Post. iii, are offered as exhaustive? If not, the claim of that postulate to be a postulate is disallowed.

On examination of detail, we find that every change in imagery proposed by Newton does indeed fall within the array of classes recognized under Post. iii. But it also shows, that, unlike the transition from Copernican to Keplerian imagery, whose innovation falls exclusively under but one of these classes (a_1), the transition from Keplerian to Newtonian imagery introduces at least one change falling under each of the classes (a_1) and (a_2) there listed. Thus (a_1) Newton replaces the determining law of the Keplerian image in such wise that, instead of the elliptical orbit Kepler's first law prescribes for all planetary motions, Newton's law of universal gravitation requires the planets to describe paths not easily pictured, but most easily (it is suggested) "as ellipses whose elements continually change; . . . [in such wise that] if we regard the planetary orbits as elastic hoops on which the planets slide, and then think of the hoops as continually changing sizes, shapes, and positions, we shall have a physical picture which roughly resembles the analysis."[1]

Again, whereas Kepler's second law supposes the ratio A/T (where A is the area swept out by the radius vector [SP] in the time T) to be constant, Newton's law requires this ratio to be subject to alternately positive and negative acceleration.

Thus the Newtonian image differs from the Keplerian not only (a_1) in pattern, but also (a_2) in point distribution since

(a_1) the Newtonian adds to the structural properties recognized by Kepler (velocity and inertial mass), a third property, new to history, *gravitational mass*, and (a_2) the Newtonian point distribution differs from the Keplerian in that, whereas two points, S, P (centers of inertia) of Sun and Planet, sufficed Kepler to image as closed the system composed of the sun and any one planet, Newton requires, beside S, as many points $P_1 P_2$ P_n (planetary centers of gravity) as there are planets in the solar system For Newton's Law can recognize no less inclusive a system of bodies than our solar system to constitute a system even *sufficiently* closed to be approximately represented by a mechanical image As for any *absolutely* closed system to be found in Nature, the very wording of Newton's Law excludes the possibility of its existence, for, if "every body in the Universe" is to attract every other body therein with whatever force, no remotest mass can be without some accelerating influence or any given mass in all the world

We see, then, that neither is an array of possibilities more inclusive than that displayed under Post III (A) needed, nor would any less inclusive suffice to accommodate all the devices that Newton employed to overcome the obstacle set by the breakdown of the Keplerian method of determining a distance [SP]

From the fact that in the transition from *Kepler to Newton*, change in point distribution appears only in conjunction with change in pattern of imagery, one may be led to wonder whether distribution-change without pattern-change could ever be effective in replacing an image to which data could not, by one to which data could be adjusted A later episode in post Newtonian astronomy quickly resolves this doubt Just as in the transition from Copernicus to Kepler, a device falling within class (a_1) alone

was sufficient to effect such a replacement, so in the circumstances that led up to the discovery of Neptune, a device falling within class (a_2) alone is shown to be equally effective "One of the most dramatic events in the history of science," is an astronomer's estimate of this discovery Let him tell something of its story —

"In 1781, William Herschel discovered the planet Uranus while carrying out his project of examining every object in the heavens within reach of his telescope The elements of the orbit of the new planet were promptly determined and its motion was carefully followed by observers In a few years it became necessary to take into account the perturbations of its motion by the other planets Instead of there being perfect agreement between theory and observation, as had been confidently expected, by 1820 there were unmistakable discrepancies between them

"By 1820 it was suggested that the unexplained peculiarities in the motion of Uranus might be due to the attraction of a more remote and wholly unknown planet The problem was to find the unknown planet The mathematical difficulties involved in the problem were enormous, in fact, Sir George Airy, Astronomer Royal of England, expressed the opinion later than 1810 that the problem was wholly unsolvable But it was attacked and solved independently by two young men who were fired with the courage and the enthusiasm of youth

"One of these young men was J C Adams, Cambridge, England, the other was U J Leverrier, of Paris, France Adams finished his work first, but the work of Leverrier led to the discovery He communi-

cated his results to a young German astronomer, J. G Galle, who received the letter on February 23, 1846, and impatiently waited for night and the stars When the twilight finally faded, he turned his telescope to the sky and found the unknown body within half a degree of the position assigned to it by Leverrier, which agreed substantially with the results obtained by Adams

"Although its [Neptune's] distance is so great that more than four hours are required for its light to come to us, yet it is bound to the remainder of the system by the invisible bonds of gravitation The reasoning of Adams & Leverrier followed these tenuous threads out to the planet beyond the known borders of the solar system, and their work in doing so will always stand as a monument to the perfection of the theories of the motions of the heavenly bodies "[2]

A last episode to be included in this brief review of history, is so unimpressive as compared with those preceding it, that one might think the end of economy could well be served at little loss to our understanding of experimental method by omitting all reference to it But the episode owes its inconspicuous place in the history of astronomy to the very reason that gives it importance in the history of metric science in general it introduces a device that saved from need of replacement an imagery threatened with breakdown under the strain of accomodating new data, apparently inadjustable to any image of that pattern It tells of Bessel's recognition of a "personal equation" In 1822, Bessel had before him a record of time-observations made by different observers, each observation giving the time at which each of these observers noted the

occurrence of one and the same event (the transit of a
heavenly body across the crosshairs of his telescope) One
would not expect the times recorded by a number of
observers of an identical event to be identical, no new
problem would have been raised had, throughout a series of
such simultaneous time-observations, the different time-
recordings been found "randomly" distributed about their
means But they were not so found, of a certain three
observers, A, B, C, A's times were systematically later than
B's, and systematically earlier than C's The result was only
to be interpreted in one of two ways either one observer
was "right," the other two "wrong", or the observations of
at least two were subject to a "systematic" error, varying
with the observer in magnitude or direction (from the
mean) The former had been the accepted interpretation
before Bessel's time, subject to the obvious embarrassment
of deciding which observer was right and which wrong. The
latter interpretation was proposed by Bessel, and soon
accepted by the community of astronomers Bessel, namely,
recognized every observer to be affected by what he called
a "personal equation "

Generally, one would expect some interval to elapse
between the moment at which an event had acted on an
observer as a stimulus and the moment at which the
observer had reacted to the stimulus by recording his
observation But would this interval, this "time-lag" be the
same for all men, or would it vary from observer to observer?
Experiment showed it variable But though varying from
man to man, would it not, for any one man, be invariable
from moment to moment? A first ready assumption, that
the time-lag was a personal constant or near constant, was
quickly abandoned, as the laboratory results of that new
type of scientist, the "experimental psychologist" (or, in

this particular *connection*, the "psychophysicist") came to be studied. They showed that one's "reaction time" (as it came to be called), so far from being constant at all times and under all conditions, varied widely with the experience anteceding and environing one's operation of perceiving and recording the time of an occurrence. It could even change sign and from a "lag" become an anticipation: in a way to justify such colloquial phrases as "going off at half-cock," "jumping the gun," etc.

Not only was the "most probable reaction time" that had conditioned any given record of (say) a transit, extremely difficult to calculate, but, as experimental psychology developed, it was recognized that reaction time was not the only "personal equation" to be estimated and allowed for in bringing into accord apparently incompatible observations made by different observers of one and the same phenomenon. So far from it, that it is no longer safe to suppose any *number* of personal characteristics *now recognized* as affecting one or another kind of observation to exhaust all those to be taken into account in "harmonizing" the discordant observations of different observers of an identical happening.

Into all this bewildering complexity of problems facing the science that would establish an "approximation to fact," even to a fact so apparently easy of approach as the angle between two lines or the distance between two points, we need not enter for the moment. *It is enough to jot down for future consideration a question ancient science could not ask, and modern science cannot escape; whether progressive approximation to physical facts is not as dependent on progressive approximation to psychological facts as the latter is dependent on the former?*

However that may be, this last episode in our review of

historic astronomy will have served the immediate purpose
for which it was introduced, in settling one question It
shows the devices by which science may replace broken-
down methods of measurement, to be not exhausted by
those falling under the head of (A), the first of the two
classes listed under Post III Bessel's recognition, and
science' subsequent use of a personal equation in the form
of reaction time is to be classed not as (A) a revision of
imagery to accommodate data, but as (B) a revision of the
method of adjusting data to imagery

The importance to all metric science, if not to all science,
of the role assigned, from Bessel's time on, to the personal
equation in all its many forms can hardly be overstressed
The process, commanded in Post II, of gathering new data
from which to construct new measurements to be added to
those previously made by an old accepted method, is of
course endless Every prospective prolongation of this
process holds a double threat to that particular adjustment
of data to image which, having successfully met the require-
ment of Post I, has made the method of measurement based
on that image available for purposes of approximation The
first of these threats has already been noted new observa-
tions made on any limited natural system are bound to
observe this system in new relations to the outlying regions
of Nature, one recalls how dependent on a planet's distance
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historic astronomy will have served the immediate purpose for which it was introduced, in settling one question. It shows the devices by which science may replace broken-down methods of measurement, to be not exhausted by those falling under the head of (A), the first of the two classes listed under Post. iii. Bessel's recognition, and science' subsequent use of a personal equation in the form of reaction time is to be classed not as (A) a revision of imagery to accommodate data, but as (B) a revision of the method of adjusting data to imagery.

The importance to all metric science, if not to all science, of the role assigned, from Bessel's time on, to the personal equation in all its many forms can hardly be overstressed. The process, commanded in Post. ii, of gathering new data from which to construct new measurements to be added to those previously made by an old accepted method, is of course endless. Every prospective prolongation of this process holds a double threat to that particular adjustment of data to image which, having successfully met the requirement of Post. i, has made the method of measurement based on that image available for purposes of approximation. The first of these threats has already been noted: new observations made on any limited natural system are bound to observe this system in new relations to the outlying regions of Nature; one recalls how dependent on a planet's distance from another planet was the noticeability of planetary perturbations. The second threat is forced on our attention by Bessel's contribution to experimental method: as indefinitely prolonged observation of the same object cannot be compassed within the definitely limited life of a single observer, it is not accidental but inevitable that the command of Post. ii address itself to an endless succession of individual observers, each affected by his own personal

instead of 42°, or mistakes in measuring an angle by sighting to the wrong signal "[3]

And of course there are as many possibilities of errors of this kind as there are varieties of instrument to be read

In any series of measurements, any one may have, but the one showing the maximum deviation from the mean, is the one most likely to have used as datum a mistaken reading of this kind Where this maximum error is large enough to create a suspicion of misreading, is not the scientist justified in excluding the measurement exhibiting this error from the set in which it appears? Not, certainly, on his individual responsibility, uncontrolled by any test of the probability of his suspicions being true to fact But mathematicians have contributed more than one criterion, based on probability theory, by which the likelihood of a misreading being responsible for an excessive error may be measured, and therewith a limit set to deviations from the mean not attributable to mistaken readings If, then, the error be greater than this limiting amount, the measurement affected by it is to be excluded from the set into which it enters, and a new approximation calculated from the measurements that remain The simplest of these criteria was furnished by Chauvenet, a footnote setting down its result will give sufficient indication of the factors entering into its computation [4]

In passing, let us remark as a matter of future and very general interest, the implication this "rejection of data" holds for the meaning of a term constantly reappearing in the various schools of philosophy, the *immediately given*, i e the "ultimate datum" of experience In the course of the present discussion, it has been pointed out more than once that the ultimate data furnished for all purposes of measure-

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ment and approximation were *taken to be* instrument readings. The example just furnished of the systematic rejection of data *taken to be given*, when so taking them leads to (what for the moment we may vaguely call) unacceptable results, carries with it a suggestion threatening to the primary assumptions of most schools of philosophy. If the implication of our technical example holds in general, then there is no such thing either in general experience or in exact science as an immediately and irrevocably given datum. Yet, one recalls, it was as no other than such ultimate and unquestionable data that Locke pointed to his "simple ideas"; Kant, to his *reine Empfindungen*; and, had our review of history included Fichte, to his *Absolutsubjective*. There is nothing in the methods of experimental science to encourage an acceptance of ultimate data in any other sense than the one here exemplified: in every operation science must, indeed, *take* something as *given*, in no operation does it promise to keep on taking that thing as given. Of historic attitudes toward *the given*, science' own "given" comes closest to that expressed by Hegel in his characteristic saying, "die Unmittelbarkeit ist selbst Reflexionsbegriff." (*The immediately given is itself a conception born of reflection.*) We shall have occasion later to consider this matter in a more general context and more systematic way.

We see, then, that data-revision enters into the practice of experimental science in two different ways: data-correction, and data-rejection. In the first, all instrument readings are preserved, but preserved as variables associated with others in a function whose calculated value is adjustable to an imagery to which the instrument readings alone are not to be adjusted. In the second, instrument readings previously taken to be data, are rejected as having been mistakenly so

taken. Of the part played by data-revisions of the first kind in averting a threatened breakdown of science' ability to meet the demand of Post. i, without revision of imagery, Bessel's use of his "personal equation" gives us one out of many possible historic examples. Of data-revisions of the second kind, as many examples could be given as one could cite occasions of which a metric scientist had applied Chauvenet's, of some like criterion to the exclusion from an irresponsive series of measurements of all those affected by excessive error. If the measurements remaining after all exclusions made, constitute a responsive set, then all observational data entering into these measurements will be shown to have been properly adjusted to whatever mechanical image the method of measurement used had accepted as the basis of its computations; computations, namely, by which, from measurement taken at one moment, the value of the measured quantity at another moment was determined.

The wording of Post. iii (B) will now be understood to have this reference:

(B). Revision of data, either

   (b_1) as to method of adjustment (revision by *correction* of data), or

   (b_2) as to data to be adjusted (revision by *rejection* of data).

Summing up the achievements of this brief logico-historical survey of the past we may accept this much to have been shown: in whatever uncertainty our scant review of history may leave us as to the exhaustiveness of its classification of the devices available to science in the way of "determining" such fundamental attributes of bodies as angles and distances, it affords us assurance of at least this much: if the classification of such devices under Post. iii is not exhaustive of all possibilities, no less inclusive an array

could be For, looking back, we see that a device falling under each one of the four classes, (a_1) (a_2) (b_1) (b_2), has entered into at least one operation of adjusting data to imagery, which no combination of devices falling under the remaining three classes could have effected without it

For the rest, the meagreness of the historic material here reviewed need not leave one too skeptical as to the generality of the results arrived at If, indeed, we were assured that of two sciences as remote in their interests as (say) astronomy and botany, the one was and the other was not required to adjust its data to mechanical imagery, or if, having this need in common, they had no common method of meeting it, then we might well feel that time spent in analyzing the postulates and methods of any limited array of sciences was time wasted, so far as any hope of finding postulates and methods of meeting them common to all science was concerned But should we find that botany no less than astronomy stood in need of effecting such an adjustment, that, further, a revision of the data to be adjusted was a method available to both for the satisfaction of this common need, and that, finally, the apparent unrelatedness of the two sciences lay rather in the dissimilarity of the "data" they respectively "took to be given", than in the use to be made of these data, then—what?

But whatever the validity our developing study may accord to the hypothesis raising this question, and whatever answer it may suggest to the question raised, we must rest content for the moment with what it has been able to gather in the way of resources *recognized by all* to have been successfully used in overcoming difficulties besetting science' progress toward (what we have called) one of its ideals It is time, after allowing ourselves a brief moment of retrospect, that our study face those difficulties which many

claim not to have been overcome, to be in fact beyond possi-
bility of resolving, by any of the methods available to
experimental science As most of these claims have been
made by many minds as spread through many ages, they may
be counted on to engage our study for some time to come

¹F R Moulton, *Astronomy*, Macmillan, 1931, p 218

²Moulton, *op cit*, p 219

³Merriman, *op cit*, p 4

⁴Let n be the number of measurements taken, p, the probable error
of a single measurement (cf supra, p 105), t, the value furnished by a
precalculated table (analogous to a table of logarithms) corresponding
to the number of measurements, n Then, if x represents the maximum
error that does not indicate a probable misreading of data,
$$x = tp$$
If then the maximum error of a given set exceed this limit, x, the
measurement exhibiting this error is to be excluded, and a new mean
and probable error calculated from the remaining set of (n—1) measure
ments This procedure is of course self repeating, and is to be continued
until a set remains whose maximum *error* does not exceed the value it
yields for critical limit x Merriman *op cit* p 108 (Editorial note
Here again there are technical problems which this account ignores,
but these are not of importance to the point of the discussion)

16. Retrospect and Prospect

OF THE FOUR RULES DESCARTES LAYS DOWN FOR THE conduct of his own thought, none has been more approvingly quoted or less faithfully followed than the last "In every case to make enumerations so complete, and reviews so general, that I might be assured nothing has been omitted"[1] And in fact, to check one's eagerness to meet what adventures he along a vaguely charted way ahead, to turn one's eyes back on a territory already traversed, takes some self-discipline Yet Descartes is right, it is wise to do just this, not once but often, not so much, indeed, to see that "*nothing has been omitted*," as to take count of how much has been, and in some part, must continue to be passed over For, as the eye of the traveler ascending a rough acclivity, is too intent on picking a way for his feet, to take in more than a narrow ribbon of the country bordering his path, so is the thinker, seeking a way through difficult issues If, on attaining some little eminence open of view, he turn to survey the land through which he has passed, he will see it as a vast multi-patterned fabric, of which his traversed path is but some single thread Not until he shall have reached this eminence is a broad landscape open to him, but not until he shall have followed some straight and narrow way can he have reached the eminence Descartes' counsel is sounder than he knew, what profit a thinker's toil may have earned for him is only to be gathered in retrospect

But how gather it, even in retrospect? In no ordinary

sense of the word would Descartes' proposed "enumeration from which nothing has been omitted" be possible, nor would a more cursory one be profitable But there are two notes we might hope to enter in our travel journal, which if carefully considered should prove of value for future reference If, in the first place, our way has seemed narrow, it is because our attention has been fixed on a single "sample" of what we took to be a wide class of problems susceptible of like solution, can we not now form some estimate of the generality of this class? Again, if the results seem meagre, it is because they have recorded only the summa genera of the methods by which the scientist's problem might be solved, can we not "enumerate" in some detail the various species these genera subsume?

In the way of generalization, it is only to make explicit a principle frequently suggested in previous pages, to set down our understanding that the demands there recognized as necessary of fulfilment in determining an angle or a length, are to be met in determining any coordinate or property of bodies given in Nature

This is perhaps obvious enough, where the properties in question are metric, i e properties to be measured in terms of the units of some continuous scale Such properties are either one dimensional (which we shall call *primary*) or multi-dimensional (which we shall call *secondary*) Primary, for example, are length, time, mass, charge, magnetic pole, secondary, such mathematical functions of the primary as velocity, acceleration, force, energy As in all such functions, length and time appear as variables, it follows that whatever conditions are to be met in determining a line or angle, are to be met in determining any other metric property

It is not so obvious from anything previously said, that

the same principle holds for statistic properties, i e properties to be determined by counting discrete units What these properties are, and how determined, has not yet been studied It is then to anticipate later results, to set it down that all statistical determinations involve the counting of discrete individuals constituting a manifold subject to some common condition But among the conditions to be fulfilled by any manifold subjected to statistical investigation, some are always set in terms of metric properties E g if the manifold to be investigated by statistical method be limited to human beings, among the requirements to be met by the biological species *homo sapiens*, some would be "morphological" (i e structural, and so, metric) in nature, since all biological classification into genera and species is based on considerations of morphological likeness and difference So that, whatever postulate is to be met in determining metric properties, is to be met in determining whether an individual does or does not conform to the morphological conditions imposed on any manifold to be investigated by statistical methods

And there is another generalization within our reach at this point, which, if it end in broadening the application of a metric method previously discussed, does so by first lifting from the implication of that method restrictions special to the limited example so far offered of its use It is generic to all processes of arriving at approximations of metric attributes, that they are *calculated* from a responsive set of measurements In our discussion of a test of responsiveness (Chapter 10) a footnote explained that our analysis would have to confine itself to the requirements imposed by the "classic" application of probability theory to the problem here involved In this classic application, a most probable value and the probable error with which it was affected

could be calculated from a set of measurements, only if the frequency distribution were one to which a "Gaussian" probability curve could be adjusted But the metric scientist of today is no longer restricted to this classic requirement From the appearance of Karl Pearson's work on, statistical theory has recognized an ever increasing variety of frequency distributions from which approximations of determinate order could be calculated Only a methodologist confining his attention to this particular problem of statistical science could afford to enter into a discussion of the present limits and possible future expansion of the genus of frequency distributions from which the statistician can wring approximations of determinate order Nor are the "simple" metric problems of determining an angle or a length unconcerned with these developments of statistical method It is unlikely that any, unbelievable that *all*, determinations of such "personal equations" as enter into the "correction" of data to be adjusted to imagery should be effected, without the need of calculating most probable values and their probable error from data whose frequency distribution is anything but "classic"

It will be seen, then, that, narrow as has been our way, it has led us to an eminence which, however modest, commands wide horizons, and however small the examined sample of the territory these horizons enclose, it really is a sample, since what important things have been found to hold for it, hold for any other sample that might be taken

So much in the way of generalization, what can be said in way of specialization? The devices by which science might replace a method of measurement that under the test imposed by Post 11, has revealed its inadequacy to meet the requirement of Post 1, have been subsumed under the

broadest possible genera available for their classification
Can we not, within manageable limits make out something
of the variety of species these genera include?

Consider the first class of devices by which a lost ability
to satisfy Post 1 may be recovered, namely,

A   Change in the image to which adjustment is to be made,
either

    (a_r) In pattern, or

    (a_s) In point distribution

Of the purely formal possibilities such changes of imagery
open to our imagination, enough has been said in the past
to let a brief repetition of them serve as premise for an
orienting corollary Any formal mechanics, such as would
set the design of a schema to which any imagery accepting
that mechanics must conform, presupposes certain other
formal sciences of which each, in the order of their mention,
presupposes its successor kinematics, geometry, arithmetic,
logic "Presupposes" means that each in the order stated,
accepts as a premise from which to deduce its theorems, at
least one *postulate*, introducing at least one *indefinable* not
appearing among the propositions and terms of its successor
Thus, if a mechanics accepts as one of its premises, Newton's
law of inertia, and therewith admits into its vocabulary the
indefinable *inertial mass*, it must, for the formulating of its
postulates, deductions and theorems, make use of some
formal kinematics, of which the law of inertia is not a
premise, and in which the term *mass* does not appear Con
tinuing through the sequence of sciences, one notes that
kinematics introduces the "dimension" *time*, which does
not appear in (space) geometry, geometry, the dimension
distance (in metric) or *straight* (in projective) which does
not appear in arithmetic, arithmetic, the term *number*, not
used in logic [2]

This ordering of the formal sciences of which any formal mechanics must make use, suggests a convenient order in which to survey the historic differences of mechanical imagery; namely, to proceed from such differences of pattern as affect mechanics alone, to those that involve differences in kinematics as well, then to those that reach to the depth of geometry. So ordering our survey of history, we should expect, and we shall find, the shallower variations of pattern to have had the longest history and widest range; the deeper ones, the shorter history and more limited range.

Any review of the long past of mechanical imagery must begin with the Democritean model of rigid, unbreakable atoms, moving in void, under laws that could only have been laws of inertia and of impact. From this, our thought turns to the Cartesian variations,[3] still atomic, still admitting only laws of inertia and of impact, but discarding the Democritean conception of the void. Next, the great Newtonian innovation: an imagery that adds to the classic laws of inertia and of impact, a law implying action at a distance, the law of universal gravitation. Yet Newton is not prepared to discard these classic laws, with the result that his imagery presents a picture of Nature subject both to gravitational forces acting at a distance and to elastic forces acting only through contact.

An imagery in which two such contrasting types of law play indispensable parts could not fail to challenge the imagination of a mind so wedded as was Newton's to the principle of "parsimony." In the course of discussing the *Quaestiones* appended to the third book of his *Optica*, Newton is led (Quaestio XXXIII) to consider the possibility that the gravitational forces assumed in the *Principia* will prove to be only one of the many types of action at a distance implied in the manifold operations of Nature. "The

attraction of gravitation, of magnetic, and of electric force, reaches out over sufficiently large intervals and therefore naturally has fallen under the eye of observation, but of course it may be that there are others of such kind as to be contained in regions so minute as to have so far escaped all observations" [4] Yet, in spite of this broad appeal to actions at a distance, Newton is never content to accept such actions as ultimate explanations of phenomena He is never willing to exclude the possibility that apparent actions at a distance may turn out to be the effects of real though concealed actions through contact In this same Quaestio XXXIII, his suspicion of a final unity underlying the apparent duality of natural forces finds its complete expression "By what efficient cause," he writes, "these attractions be effected, I ask not (*hypotheses non fingo*) What I call attraction, may well enough be brought about by impacts, or in some other way unknown to us   "

One has only to let this thought mature for a century, and one comes quite naturally to the formulation and exploitation of each of the two hypotheses Newton "would not venture," but suggests as prescribing the only possible ways in which his mixed image of nature might be simplified The simpler conception is that which Le Sage, in his *Newtonian Lucretius* communicated to the Berlin Academy, 1782 [5] There is no need to recall the argument of this famous attempt to resolve the most important example of apparent *actio in distans*, gravitation, into a phenomenon of real though imperceptible impacts, in the end gravitational phenomena are made to appear the effect, not of a force of attraction drawing bodies together, but of innumerable forces of impact pushing bodies together

Meanwhile, Newton's second, and more subtle suggestion, had inspired a work whose lengthy title fully reveals

its motive: Boscovich' *Philosophia naturalis theoria redacta ad unam legem virum in natura existentium.*[6] Here we find the "one law of force" to be "such that for very minute distances the [reciprocal] forces are repulsive, increasing without limit as the distance decreases without limit; so that, however great the velocities with which two points approach each other, these forces are capable of reducing them to zero before the distance between the two points shall vanish." Then, these repulsive forces, having reduced the velocities of approach to zero, impart to the two points velocities of separation; but "as the distance of separation increases these forces diminish until at the end of a certain interval they will have gradually faded to nothing; after which, as the distance continues to increase, the forces continue as attractions." These attractions in turn diminish with the distance, pass through zero, change into repulsions; and the story repeats itself as often as the explanation of phenomena may require. But the phenomenon of gravitation requires that the story of change-of-sign do not repeat itself indefinitely; so we find that "after the points have reached a sufficient distance of separation, they fall under forces of attraction that diminish with the distance after a law not sensibly different from the law of inverse squares; and so continue for all distances without limit, or at least for distances far greater "than those of the planets and comets."

Such were the Eighteenth Century's most carefully worked out variations in pattern of mechanical imagery; neither, forced by the need of adjusting new observations to older patterns inhospitable to them; both, inspired by that persistent craving for simplicity, or parsimony, that runs through the whole history of the *art* of image making. Neither simplification, whether in the form originally presented by its proponent, or in any of the numerous modi-

fications of it in Nineteenth Century writings, could find sufficient empirical confirmation to justify its own acceptance to the exclusion of its rival's. Helmholtz, in his *Vorlesungen*,[7] feels quite at liberty to retain, at least for reasons of practical convenience, the "mixed" imagery of Newton.

Thus, at the close of the Nineteenth Century a review of historic variations of imagery shows mechanical images to have differed widely in the mechanics of their proposed patterns; but what of the underlying postulates of kinematics and geometry presupposed by their respective mechanics? Examining them, we find that while variations in kinematical postulates do occur, yet these variations are only such as to leave unaffected what we shall presently characterize as the "more fundamental" postulates of that science; while in the geometry presupposed by their respective kinematics they differ not at all.

The relative unimportance of such differences in kinematical postulates as the history of imagery records, up to the end of the Nineteenth Century, is more readily seen if viewed in the light of all differences that *could* differentiate kinematical postulate sets. And the method of effecting an exhaustive classification of all possible kinematics is quickly told.

Letting x_i be any one of the space coordinates, $x_1, x_2, \ldots x_n$, and t the time coordinate of a moving point, the postulates of any formal kinematics confine themselves to imposing certain conditions on the time derivatives of the function

$$x_i = f(t)$$

If, now we let $f\nu$ stand for the νth derivative with the understanding that $f^0(t) = f(t)$, then all postulate-sets are differentiated by the value they assign to ν in the proposition,

"All derivatives of an order lower than the νth are continuous and single-valued functions of t"

In saying that none of the variations of mechanical imagery appearing up to the end of the Nineteenth Century implied "fundamental" differences of kinematical postulates, the exact meaning of "fundamental" may now be given In none of these imageries did their presupposed kinematics assign to this νth derivative an order lower than the second That is, none admitted the possibility of a moving point (1) suffering an instantaneous change of position or of velocity or, (2) losing either position or velocity at a moment lying between two moments at which it possessed both Of possible attitudes toward derivatives of higher order than the first (velocity), it is enough to note that both limiting cases are historically represented Helmholtz accepted the possibility of instantaneous changes of acceleration, Boscovich denied the possibility of instantaneous change in a derivative of any order In neither case is the argument adduced by its proponent for the position taken cogent enough to exclude the possibility of reconciling the general type of mechanical pattern accepted by their respective postulates with other postulates of kinematics Helmholtz, making no attempt to save the principle of parsimony by eliminating one or the other of the two laws determining his pattern of imagery (gravitation and elasticity), simply finds it convenient to assume that instantaneous changes of acceleration characterize the moment at which both forces begin to affect the point simultaneously Boscovich bases his position on the "logical contradiction" implied by the assumption that a point could have two derivatives of any order at the same time He urges that, to attribute to a point two velocities differing in magnitude or in direction, would mean that the point was about to

travel in two different paths or with two different speeds at the same time. The fallacy of this reasoning is frequently repeated in Eighteenth Century systems, and is doubtless to be attributed, in part, to the Leibnitzian influence, with its insistence on continuities; in part, to the difficulty of forming one's thought to the full meaning of the new mathematical method introduced (on the continent) by the Leibnitzian form of differential calculus.

But if the varieties of imagery developed up to the end of the nineteenth century, different as they are in their formal mechanics, present only minor differences in their underlying kinematics, and none at all in their space-time geometry, the Nineteenth Century itself did suggest new possibilities touching the "deeper" postulates of mechanics, some of which found important application in the Twentieth Century. Thus, in the matter of kinematics, a paragraph in the general review of possible patterns of imagery with which Hertz introduces his *Prinzipien der Mechanik* (posthumous, 1894) is sufficiently daring in its suggestiveness to be reproduced in the notes appended to the present chapter.[8] But of far greater importance to later developments in mechanical imagery was the demonstration by Lobachevsky, Bolyai, Riemann and others, of a possibility that mathematicians from the time of Proclus on had supposed themselves to have proven impossible: the possibility, namely, of constructing self-consistent geometries whose postulates should contradict at least one of the postulates of Euclid.[9] It remained, however, for the Twentieth Century to attempt the construction of a formal mechanics whose differences from all earlier patterns reached to the depth of their fundamental kinematics and, finally, to their very geometry. To this effort, the image makers of the Twentieth Century were driven by what

they took to be the inadjustability of certain new observations to any previously available pattern of imagery.

In the matter of kinematics, it will be enough to consider the most revolutionary of these Twentieth Century proposals the rejection of the most fundamental of all traditional postulates, that which denied the possibility of instantaneous change of position   To accommodate the observed "sudden" jump of an electron from one orbit to another, more than one postulator proposed the interpretation of this empirical "suddenness" as mathematical "instantaneity"   i e   the attribution to the same material point of two positions at the same moment of time   The proponents of this innovation (generally referred to as "discontinuity of space-time path") seem to consider it sufficient defence of the consistency of their proposal with all other postulates of mechanical imagery, to point out that it is no more difficult to construct discontinuous functions entering into the equation $x_1 = f(t)$, than it is to construct such functions for the equation $d^2 x_1/dt^2 = f''(t)$ (expressive of the instantaneous changes of acceleration sanctioned by Helmholtz)   And this of course is true enough, there would be no difficulty in plotting a curve representing a discontinuous space-time path of a point *assumed to be the same point*, but how know this assumption to be in accordance with fact, when all previous history had recognized but one assurance that differently dated observations made on two points were really made on one and the same point at two different moments of its history? This assurance was based on the acceptance of evidence sufficient to show the point to have traveled a continuous space-time path from its first to its second space-time position   It is Helmholz' conviction that no substitute could ever be found for continuity of space-time path as a *principium identitatis*, that moves him

to word and argue his first postulate of kinematics as
follows

> "First, the coordinates of position, x, y, z, must be
> continuous functions of time, t This condition is indis-
> pensable to any recognition of the identity of a material
> point at different moments of time, for if the coordi-
> nates, or any one of them, were at any moment to
> change with a leap, the path of the material point
> represented would, at one and the same moment, come
> to an end at one point of space and begin again at
> another finitely remote from the first, without having
> traversed any path connecting the two In such case,
> we can imagine no means by which the identity of the
> material point before and after this leap could be
> established but would suppose ourselves in presence of
> a phenomenon contravening the deepest axiom of all
> experience, namely, that matter can neither be
> generated or destroyed "[10]

So Helmholtz, the confirmed Empiricist, would argue the
matter to deny this first postulate of kinematics is to
"contravene the deepest axiom" or "best confirmed induc-
tion", of all experience A Kantian would have based his
acceptance of this postulate on a quite other and, as it would
seem to him, more cogent argument To deny a proposition
supported by the best experimental evidence is a serious
matter, to deny the possibility of gathering experimental
evidence either supporting or refuting the proposition is a
still more serious matter That to deny the postulate of
continuity of path is to deny to science a condition that
makes experimenting possible is a conclusion to which
Criticismus is inevitably led for reasons we have already

met. (Chapters 3 and 4.) Accepting, with Helmholtz, the premise that path-continuity is not only the one historical but the only conceivable *principium identitatis*, and remembering that all experimental evidence is based on repeated observation made on one and the same object, it follows that, to deprive science of the one criterion by which the identity of a body could be established is to deny it the possibility of gathering evidence of any sort. But if we accept the Helmholtzian premise, must we not accept the Kantian conclusion? And does not this conclusion establish our postulate of kinematics as an *a priori* truth; since it could not be refuted by the evidence of experiments that could not be made?

The question carries our thought back to another, mooted in an earlier chapter of the present study (Chapter 5). Granted that a *principium identitatis* is presupposed by every experimenter who would make a series of observations on the *same* body; granted, even, that any such *principium* must accept the postulates of *some* space-time geometry, must the space-postulates assumed be Euclidean, and the time-postulates be Newtonian? It is easy to see the motives which would have induced one of Kant's day to return a prompt and confident affirmative; it is not easy to find these motives compelling today.

It was precisely because Kant, with all of his day, took Euclideo-Newtonian geometry to furnish the only conceivable method of setting up the individuating frame of space-time coordinates by which the path of a moving point could be traced that he took the postulates of that space-time geometry for *a priori* truths. They had for him the inevitableness of *Bedingungen die die Erfahrung möglich machen*. But the century following Kant's showed him and his time mistaken: there were other geometries that, if

needed, could be used to set up Non-Euclidean space co-
ordinates of an individuating frame; and the century
following that—our own—has liberated science from its
subjection to the Newtonian conception of the time-
coordinate. May not Helmholtz, with all scientists of his day
and most of our day, be mistaken, too? May not science
develop another *principium identitatis* than that of path
continuity?

If such thought as the present writer has been able to
concentrate on the matter has left him less confident than
was Helmholtz that no *principium identitatis* consistent with
a discontinuity of space-time path could possibly be devised,
at least his search of literature leaves him without example
of a formal kinematics in which this possibility has been
realized. All that we may accept as settled in the matter is
the perfectly obvious point that, *unless* a set of postulates can
furnish a *principium identitatis* dispensing with the require-
ment of space-time path continuity, the random suggestion
of possible path-discontinuity contributes nothing to the
enrichment of the patterns of imagery to which observa-
tional data might be adjusted in satisfaction of Post. i.

Although our study has thought it well to make as
systematic as possible its review of a past whose story it is
about to leave, none of the reforms of mechanical pattern it
has so far considered touches in depth that most recent one
which is both the wonder and the bone of contention of our
day: the Einsteinian theory of relativity. Here at last the
Nineteenth Century suggestion of non-Euclidean geo-
metries has born full fruit. In the Einsteinian image, the
possibility of "deep" variations of pattern has touched the
ultimate depths; it replaces the whole Euclideo-Newtonian
space-time frame common to all previous patterns, by a
frame constructed in conformity with the postulates of an

entirely new space-time geometry And this it does not
merely as an exercise of the imagination, offering (as did the
Nineteenth Century neogeometries) one or another alter-
native to a geometry in current use, should such an alterna-
tive ever be needed The studious attention with which our
century has received the "relativity theory," is attributable
to the fact that the new pattern of imagery it proposes was
offered to a world painfully aware that some change of
pattern would have to be invented, if new observations of
many different kinds were to be adjusted to any type of
mechanical image Whether the Einsteinian imagery does
now accommodate all observations inadjustable to old
patterns, if so, how long it will remain hospitable to future
observations, is of course a question whose answer lies in the
hands of specialists of this day and of days to come With the
outcome of such special investigations the present study is
not concerned, "relativity theory" will have served the one
purpose for which reference to it is here made in furnishing
the closing episode in this retrospective glance at, what we
might call, the growth of plasticity in the pattern of imagery
to which observational data are to be adjusted, if metric
science is possible Alone of all historic revisions of
mechanical pattern, this latest one reaches to the depth of
reforming the whole individuating apparatus of such
imagery—the space-time geometry of its reference frame
What modifications the theory proposes in the way of
determining law, are made possible only on condition that
this fundamental change in the geometry of its pattern of
imagery be accepted As history furnishes no example of
revisions of pattern involving changes in the formal sciences
presupposed by geometry itself (arithmetic, namely, and
logic) our retrospective view of history has come to its last
word

But if this is to be our study's last retrospective word, one may wonder how it can have come to that word, with nothing said of three of the four genera of device listed under Post III, by which science has overcome obstacles set in the way of her progress Have then the other three entered into history in no more various ways than those illustrated in our few astronomical examples? All three of them have, indeed, but in ways our study is not yet prepared to consider two of them (b_1) and (b_3) play their most notable part in the history of problems not yet introduced, the remaining device has throughout the past played the part we are about to let it play in the present study, that of introducing these new problems

This device is the one listed under Post III as (a_2) "revision of point distribution" One may have felt it implied, but it has not been explicitly noted, that the progress of science in raising lower to higher orders of approximation, presupposes a progressive revision of point-distribution always in the same direction transition from a less to a more complex point manifold Why this should be would be obvious enough, if we could accept it as a universal principle that no pattern of imagery could represent any limited portion of Nature as a permanently closed natural system That this is true of some patterns of imagery has already been shown, patterns which, like the Newtonian, Boscovichian, Kantian, require every material point within a limited natural system to be at every moment subject to a force exerted on it by every material point lying without that system But not all historic patterns impose this requirement on the images conforming with them the Democritean and Le Sagian do not These, so far from excluding the possibility of any limited portion of Nature, constituting at any moment of time a closed system,

represent every natural system as at every moment constituted of as many closed systems as there are atoms within it,
not in immediate contact with other atoms; and these
systems would remain closed until new atom-contacts had
been made. No more than the Newtonian, however, could
the Democritean pattern of imagery represent a limited
portion of Nature as constituting a *permanently* closed
system. At the end of a sufficiently long period, the impact
of two atoms now occurring at a distance however remote
from any given atom, must result in that atom being subject to some impact it would not otherwise have suffered.
Assuming, as we safely may, that what holds for these two
most contrasting types of imagery (one of *actio in distans*;
the other, of rigid connections) holds for all patterns, we
come to a conclusion that prescribes an inevitable course for
our future study. Namely, that no approximation to a metric
attribute of a natural body can be raised to an order higher
than any given order, unless the volume of the mechanical
image used in calculating that approximation be enlarged
to a volume greater than any given volume, and its vacuum
reduced to a vacuum less than any given vacuum. (Here, by
volume of an image is meant the space contained within the
minimal surface beyond which lies no imaging point; and by
its *vacuum*, the space bounded by the maximal surface within which lies no imaging point.) It is not hard to see the
challenge that such a conclusion presents to our idealistic
interpretation of *truth* and *reality*. For to approach without
limit the limiting conception which gives us our definition
of these terms, is to adjust all data, entering into an unlimited number of measurements, to a mechanical imagery
of such increasing complexity as to approach without limit
the limiting condition of an infinite continuum of imaging
points. In the course of its progress toward such an ideal, our

imagery can leave no region of space permanently un-explored and unrepresented by imaging points But if in the course of our exploration of Nature we should find incontrovertible reason for "taking as given" an object of such nature as to be inadjustable to any mechanical image, progress toward any such ideal as has furnished us with the only meaning we could accord to *truth* or *reality*, is permanently checked With that check the meaning of limiting conception defining that ideal would be as completely lost as would be that of the mathematical limit defining (say) the $\sqrt{2}$, were we to discover that some series of numbers supposed to be the only ones having that term for its limit could develop no term whose square differed from two by less than some finite number

But does our exploration of Nature "give" us nothing so natured as to put a thing of its kind beyond possibility of adjustment to any mechanical image, whatever its pattern and whatever the complexity of its point distribution? It is because such a weight of historic opinion supports the view that Nature abounds in such inadjustable data that we realize the next step in the program of study originally planned, to be both inevitable and serious

Thus it is that the device of revising point distribution, endlessly recurrent recourse to which is a necessary condition to science' overcoming one kind of check to progressive approximation, brings with its use new threats to the progress of science To be sure, the difficulty they raise is generically old to this study, the difficulty, namely, of adjusting all observational data to a mechanical image of the natural system observed The novelty that the future has in store for us, lies in the kind of data each new problem "takes to be given" as material requiring adjustment These observational data are far enough removed from the simple

instrument readings that in our previous discussions we took to be "ultimate", what they are and why "taken to be given" is matter for systematic study in the pages to come But that the term *data* should be subject to widely different applications in different contexts of discussions, is a phenomenon for which a paragraph interpolated in the argument of a previous chapter will have prepared us

If the term *data* could ever stand for the *merely given*, as in the course of history it has too often been taken to stand, the most pressing problems experience sets for reflection would be beyond solution But, in view of the relativistic interpretation which understands nothing to be *given* in experience but what, for some reason, is *taken to be given*, it cannot surprise anyone that with changing context, for changing reasons, changing things should be assigned the role of *data* With all this justifiable variation in the denotation of *datum*, there is yet one responsibility toward the thing the scientist takes to be given that no thinker accepting the role here assigned "the scientist" can escape whatever he *takes to be given* as observational datum must be adjusted to a mechanical image of the natural system in which it is observed to occur But if the scientist's responsibility toward whatever he takes to be given is always the same, so are the devices by which science may be able to meet its responsibility Now, we have already adduced historic instances in which the scientist, finding it impossible to adjust new data in the way of instrument readings to old images, and equally impossible to meet the difficulty by revising either the pattern or point distribution of his image, has nevertheless had successful recourse to one or the other of two remaining devices listed under Post iii (B), namely, (b_1) revision of method of adjusting data to image, and (b_2) revision of data requiring adjustment

With this, a plan for the discussion of the next topic which our original program of procedure charted for our study, begins to outline itself in our thought This topic was to have been an examination of the evidence on which the "indeterminists" (as we shall begin to call them) of history have based their conclusion that empirical approximation to a quantity "given" in Nature could never attain to an order higher than some finite order Now, all our past argument has gone to show what evidence to this effect would be conclusive, any evidence namely, that would establish the impossibility of meeting the requirement of Post 1, 1 e the impossibility of adjusting anything "found in Nature" to a mechanical image of the natural system in which it is "found " One can well anticipate that some at least of our historic indeterminists will claim to have estab lished just this impossibility of adjustment, and from it have drawn their inevitable conclusion Let us then begin by examining the evidence of inadjustability submitted by indeterminists of this class, should we find their evidence incontrovertible our cause is lost

Whatever the end of the story, it is plain enough with what question it must begin Of one who claims to have found in Nature this thing or that, whose existence there would preclude the possibility of adjusting the array of observational data in which this existent was included to any mechanical image of a natural system, we must ask a double question (1) are you sure you have exhausted every possible method by which adjustment might be made, (2) are you sure you have eliminated from your data every-thing mistakenly "taken to be given"? Not until we have exhausted all our reaches of experience, all our resources of inventive reflection, can we accept as incontrovertible an affirmative answer to both these questions Then and then

only shall we have to accept as final, at least *for us*, the
impossibility of meeting the demands of Post. 1, and there-
with the impossibility of indefinitely progressive approxi-
mation Only then is our cause lost, the cause, namely, of
according to *truth* and *reality* the only meaning our thought
has been able to suggest that of limiting conceptions, of
scientific ideals

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[1]*Discourse on method*, Part II

[2]Any statement of what appears among the indefinables of formal
sciences, must choose for illustration some one of the many ways in
which analysts have drawn up the postulates of the system of formal
sciences of which a formal mechanics must make use Common to all
these systems would be only the general principle, that the sciences will
be presented in some order wherein each science listed accepts the in
definables and postulates of all those which it presupposes, and adds
thereto at least one indefinable, entering into at least one postulate not
found in any of the sciences presupposed

[3]By Cartesian is here meant, not the atomic system presented by
Descartes himself, for in introducing "rational soul" points, not included
in any atom, Descartes' nature image ceases to represent a determinate
system and therewith falls short of the requirements of mechanical
imagery But these soul points were so promptly dismissed by Spinoza,
who retains the rest of the Cartesian pattern, that the adjective Cartesian
is here conveniently allowed to stand for what might more scrupulously
be called *Cartesio Spinozistic* imagery

[4]*Optica*, III

[5]*Smithsonian Report*, 1898, Part I, 139–160, Tr Sylvester

[6] First edition, 1758 (Vienna), second, 1761 (Vienna), a third, *prima
nunc ab ipso [auctore] perpolita et annota*, 1763 (Venice) We quote from
the third In the course of this volume, Boscovich makes several refer
ences to opuscles and dissertations of date considerably prior to the first
appearance of the *Theoria*, and in an opening sentence claims to have
been in possession of a *virium mutuarum theoria* as early as 1745, the
year of his *De viribus vivis* What follows in our text is a condensation
of *Theoria*, Pars I, par 10

[7]*Vorlesungen uber theoretische Physik*, Vol I "Die Mechanik"

⁸Editorial Note This appendix was not found in the author's manu
script. I hesitate to supply what might have been the author's intended
paragraph

⁹The history of the long development that culminated in the con
struction of non Euclidean geometries is admirably covered by Stackel,
Theorie der Parallellinien, 1895 The most important sources (trans
lated) are to be found in Smith (ed) *Source Book of Mathematics* 1929)

¹⁰*Vorlesungen uber theoretischer Physik*, Vol 1, "Die Mechanik "

Part III

17. New Problem Setting

HOW FREQUENTLY IT HAPPENS THAT WE TAKE FOR something new what on second thought is recognized to have been met with many times, but allowed to pass unnoticed! So it is with the "new data" whose adjustability to mechanical imagery, having been questioned or denied by many of the best minds in the past, were to furnish the topics of our further reflection they have been systematically "taken for granted" in the discussion that has gone before, but have not been thought of as among those data whose adjustment to mechanical imagery is a condition on whose fulfilment depends the possibility of indefinitely progressive approximation For, consider, could the past argument have specified instrument readings as the "ultimate observational data" whose adjustment to such imagery was demanded by Post 1, without having assumed to exist, in the same natural system as that from which these instrument-readings were obtained, an *instrument reader*? Could one have taken these readings to be data for the calculation of measurements and approximations, without having taken for given, along with the calculations, a *calculator*? Could one have adduced as the motive for these calculations an eagerness to "gain a decimal place", without having recognized this calculator to be a *desirous and purposeful being*? In short, could one have supposed there to be science, in a world in which there was no scientist?

It is not, then, new observations that furnish the "new data" whose adjustability to mechanical imagery now

challenges our attentions, but new reflections on observations already made, or supposed to have been made in all that has gone before. What these new reflections reveal in the way of things whose existence in Nature has been taken for granted in the very act of assuming "least readings" to have been taken, brings new problems to light. Of these problems, the most critical may be put in the form of a question: can the scientist meet the demands of a science that requires him to adjust his own nature to the nature of a universal mechanism? If not, how is he to define that to which his approximations approximate?

Any attempt to answer this question must begin by asking another: what, namely, is this "scientist" whose nature is to be adjusted to the nature of mechanism? Functions already taken to be essential to him suggest a few attributes that any being who is to perform these functions must be allowed to have. The scientist is to read instruments; from least readings, he is to calculate measures and approximations; from approximations of lower order he is to progress to those of higher order. But can an instrument-reader be other than a *perceptive* being; a calculator, other than an *intelligent* being; a struggler for the realization of certain objectives, other than a *wilful* being?

This is but a meager sample of the attributes all the world would accept as essential to the description of a scientist; but it is more than enough to start us on the way to deep-lying troubles. For example, does not the possession of these "*mental attributes*" presuppose the possession of others? Can any but a *living thing* have mental attributes? Can any but a *physical thing* have living attributes? Before considering whether the possession of mental attributes adds to the difficulty of adjusting the living body to the demands of a mechanical system, must we not inquire

whether the possession of living attributes does not already exclude a physical body from the possibility of such adjustment? And, for the matter of that, is our scant survey of a few favorable instances of science' adjustment of physical systems (e g astronomical) to mechanical imagery, sufficient evidence that, in the *whole domain of inanimate nature*, the physicist has found no instance in which the adjustment of the observational data to at least one mechanical image of the natural system observed proved beyond his power to accomplish, or the reach of his imagination to conceive?

The questions suggest the course our further reflections should follow, if, at each step of their progress, they would assure themselves as firm a foundation from which to set off on the next step as reasonable care can provide To afford ourselves as much security as is to be hoped for within manageable compass, the best plan would seem to be, to consider in succession three questions (1) what may the physicist have found in the world of his observing to resist adjustment to any type of mechanical imagery, (2) what the biologist in his world, (3) what the psychologist in his?

To begin with the physicist and his world, the first matter to be considered is, what is the physicist and what his world? On the one hand, what is the difference between the science of physics and the science of mechanics? On the other, what the difference between the science of physics and the science of (say) biology?

The terms *physics* and *mechanics*, as applied to presumably different sciences, are none too carefully differentiated in meaning and kept distinct in use, even by the technical writers treating of these two sciences, which have, indeed, many principles in common It is not, then, as a universally accepted meaning attached to the terms, but as

a definition consistent with their most prevalent use, and as one which establishes a formal difference between the two sciences defined, that the present study takes (1) *Mechanics* to be the science whose postulates and theorems establish (a) the principle of individuation, (b) the properties, (c) the determining law imposed on *material points* by some mechanical scheme (Chapter 15) and, (2) *Physics* to be the science whose postulates and theorems perform an analogous function for *groups* of such points (*point-group* being a term whose definition will be considered later)

As for the second differentiation of physics from other sciences, that which gives to *physics* and *biology* their specific connotations, no more need be said at this stage of our study than to name a difference to which later analysis may hope to give exact definition Namely this *physics does not*, *biology* and (what we shall later call) the *biocentric sciences* do order the bodies they respectively study into *functional classes*, i e classes of point-groups that, however unlike in structure, share a common function

Leaving for later consideration the full meaning to be given the differentia that marks off the biocentric from the physical sciences, let us first devote our attention to the difference that makes physics other than mechanics The difference between the two may best be clarified to our understanding by addressing a certain problem to our imagination how, namely, would the physical image of a natural system appear in a mechanical image of that system?

In the first place, since any physical body appearing in Nature would have to be of observable size, and therefore of finite volume, it could only be represented in a mechanical image of a natural system by a manifold of imaging points Such a manifold might or might not constitute a

point-continuum In the former case, the body imaged is said to *fill* the space included in its volume, in the latter (e g a Boscovichian image), it is said to *occupy*, without filling, the minimum volume in which all its imaging points are included The most obvious way in which a manifold of discrete points can be imagined to occupy without filling a continuous volume, is by the operation of such repellent forces between the points constituting any two bodies as to exclude each from the space occupied by the other Variations on this simple theme are open to the imagination, but need not be examined here

Next, consider the properties that might be accorded to point-manifolds representing bodies in a mechanical image A distinction has already been made in connection with point-properties that is also to be made in connection with the properties of point-manifolds Indeed, the distinction made in terms of point-manifolds is the general case, that made between manifolds composed of but one point is a limiting special case In the general as in the special case, we differentiate between

(i) *structural properties* properties of a point-manifold that are invariant with variation of its environment in a mechanical image, and

(ii) *nonstructural properties* properties of a point-manifold that vary with variation of environment

Thus, in a gravitational system, the configuration, the inertial and gravitational mass, and the velocity of the center of gravity of a point-manifold representing a physical body would be its structural properties The acceleration of this center and all properties that are functions of this acceleration (e g the gravitational force to which it is subjected) would be among its nonstructural properties, since they vary with the distance or the mass of any environing

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body of the system. In a Democritean system, the geometrical and kinematical properties of a point-manifold imaging a free-moving atom would represent the atom's structural properties; the manifold's change of velocity at a moment of contact with another atom would represent a nonstructural property.

There are a few other terms whose constant use in the sequel makes it advisable to include them among those given somewhat formal introduction at the outset. Three of these terms are adjectives differentiating certain species of structural classification into which point-manifolds may fall:

(i) a *mechanical class* is composed of point-manifolds of identical structure; i.e. manifolds having the same point-configuration and the same point-properties at analogous points.

(ii) a *physical class* is composed of point-manifolds falling under an indefinite variety of mechanical classes, but such that the properties of all their points distributively determine an identical property of all their points collectively.

(iii) a *morphological class* is composed of point-manifolds falling under an indefinite variety of physical classes, but such that all possess at least one property in which they differ from a given norm by no more than a given finite amount.

The following symbolism will be preserved throughout the sequel:

Classes	Individual class-members
i. Mechanical x, y, z	$x_1, x_2, \ldots y_1, y_2, \ldots$
ii. Physical $\mathbf{x, y, z}$	$\mathbf{x}_1, \mathbf{x}_2, \ldots \mathbf{y}_1, \mathbf{y}_2, \ldots$
iii. Morphological $\mathbf{x, y, z}$	$\mathbf{x}_1, \mathbf{x}_2, \ldots \mathbf{y}_1, \mathbf{y}_2, \ldots$

Where it is necessary to individuate in terms of both space-

and time coordinates, the space coordinate will be indicated by an arabic, the time coordinate by a letter subscript— thus x_i, x_{2i}, will have different space, identical time-coordinates, x_i, x_i, will have identical space—, different time-coordinates

Two other terms intimately associated with the preceding and, like them, of frequent recurrence in the sequel, may be included in this list of preliminary presentations, before offering examples covering the scientific use of all These terms are the pair of contradictories, *group* and *nongroup*, under one or the other of which every point-manifold must fall

(iv) If, in a manifold, (a) a property of all points distributively determines a property of all points collectively, and, conversely, (b) this property of all points collectively determines a property of all points distributively, then the point-manifold *is not a point-group*

(v) If, in a point-manifold, either condition (a) or its converse, condition (b) is not fulfilled, then the point manifold *is a point group*

Consider, now, the properties conferred upon point-manifolds by their *mechanical, physical,* and *morphological* classifications, respectively

(1) In *mechanical classification*, the distributive properties of a point manifold give it the collective property of belonging to a class x and, conversely, *the collective property of belonging to the class x determines the properties of all points distributively* Therefore, (1, supra), membership in a mechanical class does not constitute a point manifold a point group

Comment —Mechanical classification requires too close a conformity of objects so classified to mathematical conditions to be readily recognized by observation, it *cannot,*

therefore, find extensive use in empirical science. Nevertheless, it does serve the stereochemist so to classify isomers as to correlate observable differences in their physical properties with such differences in the atomic structure of their molecules as would let two isomers fall into two different mechanical classes. Thus, in the optically differentiated isomers, dextro- and levo-lactic acid, the molecules of each are so classified in terms of their atomic structures as to make the mechanical model of either molecule a mirrored image of a like model of the other. In the more recent developments of science, the most important role played by mechanical classification of structures is that assigned to it by the nuclear physicist

(ii) In *physical classification*, distributive point-properties of a point-manifold, determine a collective property in terms of which the manifold is classified, but the collective property of belonging to a class so defined, does not determine the distributive properties of the manifold points collected Hence, membership in a physical class does constitute a point-manifold a point group, the properties conferred upon it by that membership will be group properties

Comment —For example, mechanical images of bodies of the same volume, mass, temperature, will generally differ inter se in point-configuration, distribution of point-mass and velocity of point-agitation The only properties these point-manifolds would have in common are group-properties Of such group-properties, all whose "dimensions" include at least one of those which a formal mechanics adds to the dimensions appearing in kinematics, geometry, etc , are to be called *physical properties*, and any body of a class having these properties in common, however different, the mechanical classes to which the member-bodies severally belong, is to be called a *physical body*.

(III) Finally, in *morphological classification*, since bodies of the same morphological class are limited to no one physical class (and, *a fortiori*, to no one mechanical class), being restricted only by the condition that their maximum difference in physical property from a given norm shall not exceed a definite finite limit, it follows that point manifolds imaging bodies of the same morphological class will be point-groups, and the only collective properties these point-groups can have in common will be group-properties

Comment —The method of morphological classification plays a decisive rôle in the entire taxonomy of the biocentric sciences—the sciences studying the world of life, its organs, instruments, social groupings In biology itself, for example, the morphology of a given organism determines the biological species to which it belongs, while the biological species to which it belongs determines no more than the limits within which its physical properties must be confined

It is not hard to see how important a part is played in science by point-manifolds forming point groups whether of the physical or the morphological order The preceding paragraphs have offered examples of point-manifolds that owe their inclusion among point-groups to their fulfilment of condition (a) and non fulfilment of its converse (b), in these examples, one or more properties of points taken distributively determine one or more properties of points taken collectively, but not conversely

But what of the second condition whose fulfilment would equally constitute a manifold of elements a group, the case, namely, in which a collective property of the manifold determines a distributive property of its constituents but not conversely? Does science give us no example of a group so conditioned? The immediate sequel will furnish some, and later developments many such examples, but the most

important of them will be drawn from sciences whose
concern is with manifolds of which the constituents are
much more complex than points, and the group-properties
of an order quite other than physical Meanwhile, we are
not left without a familiar example from which we may
catch some anticipatory hint as to the kind of distributive
properties generally conferred on the elements of a mani-
fold by virtue of the manifold possessing a certain collective
property which could not have been determined from any
properties distributed among the uncollected elements
Take the class, *juryman* what confers upon the men con-
stituting a jury the property of belonging to this class? No
doubt, they must first belong to the class "good men and
true," but to belong to this genus is not enough to qualify
a man for membership in the species, *juryman* To acquire
this membership, he would have to possess, in addition to
the quality of being good and true, the condition of being
one of a manifold of twelve such men, chosen by a process
prescribed by law Thus, as membership in the class,
juryman, is acquired only by virtue of membership in a
manifold whose collective property determines the distri-
butive property of its elements, but not conversely, a jury is
definitely a group whose constitution depends upon its ful-
filment of the second (b) of the two conditions imposed on
element manifolds forming element-groups (but not the
first (a)—see IV, p 263 above)

Here is to be noted a difference quite general (whether or
not universal) to element-manifolds whose groupings are
consequent on their fulfilment of condition (a) but not (b)
in the one case, and of (b) but not (a) in the other

All members of a jury owe their collective weight to the
sum of their weights distributed among them, but not con-
versely All members of a jury owe their distributive

property of being jurymen to their collective property of forming a group of twelve, but not conversely One notes that the property which is determined for the group collectively by the measure in which it is possessed by the group-members distributively (weight) is quantified, that determined for the members distributively by virtue of their membership in the group collectively (that of being a juryman) is nonquantified One juryman or one jury may be heavier or lighter than another, no juryman can be more or less a juryman, nor jury, more or less a jury than another This difference will have its importance in a later context

The way in which a man acquires the property of being a juryman may be expected to play an important part in the definition of concepts that lie before us, but has it no analogue in the implications of those that lie behind us, in which the elements are the points of a mechanical image and such groups as may be formed are manifolds of these points?

There is, indeed, a collective property possessed by one manifold of imaging points that could be possessed by no other, and which does determine the kind, not the degree, of properties attributable to the points composing it It is the property of being the image of a *closed natural system* Any lesser manifold of points within this system could be so classified physically as to constitute a group whose physical properties (collective volume, mass, temperature, etc) would be determined by the distributive properties of its constituent points, but not conversely Physical bodies would be imaged by such groups, whose group formation would be due to their fulfilling condition (a) and not fullfiling its converse (b) This kind of group formation has already been recognized But now suppose it is given on experimental grounds that the total manifold before us is the

image of a sufficiently closed natural system; what does this imply as to the kind of properties all its constituent points distributively must possess? One must remember a closed system to have been defined as one in which a formulated law determines, from the point distribution of any given moment, that of any second moment. Now, our historical pages have had frequent occasion to notice how readily the mind of the past falls into the way of supposing science to discover, first, the properties of bodies, then, to search for laws, if any are to be found, that determine the behaviour of bodies so qualified. Reflection, however, makes it clear that science must arrive simultaneously at the two results: it attributes to bodies only such properties as, when substituted for the property-symbols appearing in some formulated law, establish a cause-effect relation between two momentary distributions within the same natural system; or, rather, the mechanical image of such a sufficiently closed system. E.g., only in a system whose bodies obey the law of inertia will these bodies be recognized as having inertial mass; only gravitating bodies can have gravitational mass; only bodies moving in continuous paths can have velocity, etc.

Finally, to complete the analogy between the manifold of points constituting the mechanical image, and the manifold of men constituting a jury, it will be noticed that the properties conferred upon imaging points by virtue of their membership in a manifold having the collective property of being *closed*, are determined by that membership only as to their qualities (dimensions), not as to the magnitude of their qualities. To say that a manifold of points, subject to the law of gravitation, is a mechanical image of a certain natural system, informs us that the material points of this image must have inertial and gravitational mass, velocity, and

positions determined in accordance with the postulates of a Euclidean-Newtonian geometry It does not inform as to the magnitude of any mass, velocity, or space time coordinate

It may be that, having come to the conclusion of this brief list of preliminary definitions, dry enough in themselves, yet needed throughout the sequel, one is moved to recall a sentence that introduced an earlier chapter It was to the effect that most laymen and some historical philosophies are all too ready to look upon the "facts of Nature" as so many independently given summande of which Nature is the sum Prophetically, that paragraph anticipated that, as our reflection on the actual works and words of the experimental scientist developed, we should come more and more to look upon facts of Nature and the Nature they compose as upon the organs of an organism and the organism they compose, of which neither can grow or otherwise change, save as the other grows and changes The discussion of this last paragraph may do something to illustrate the meaning and confirm the truth of this prophecy

These preliminary definitions put us in a position to offer the deferred interpretation of a *physical* as distinct from a *mechanical* image of a natural system the physical image of a natural system is one in which all observations made on gross bodies have been adjusted to any one of an infinite variety of mechanical images in which the gross bodies are represented by point-groups, certain of whose collective properties correspond with the physical properties ascribed to the gross bodies observed

Rather than encumber this differentiation with relative clauses, let a brief commentary note such of its implications as need to be understood for an understanding of the whole —

(1) In a physical image of a natural system, the cause-effect relation is not to be taken as connecting the distribution of *physical* bodies at any two moments, of which each distribution determines the other by a *physical* law. By definition, this relation can exist only between the two momentary point-distributions of a *mechanical* image, in each of which physical bodies are imaged as point-groups. To any point-group that images a given physical body in the cause-distribution, must correspond a point-manifold in the effect-distribution that images the same body. Prediction, then, from an observed physical distribution of any one moment to the expected distribution of another, is not made in terms of a determining physical law, but in terms of the mechanical law of an image to which the observations on the physical bodies in question are taken to have been adjusted. Any failure of later observation to verify such prediction is evidence that the supposed adjustment of gross observation to formal imagery is illusory; that therefore, either the image to which adjustment is to be made, or the adjustment of data to image is to be revised.

(2) In this process of readjustment, an important part has always been played by the breaking up of such grosser bodies as may have been observed, into less gross constituent bodies too minute to be susceptible of observation. Thus the molar is resolved into molecular, the molecular into atomic, the atomic into neutronic and electronic. Here for the moment our analysis of macro- into micro-bodies may halt, but nothing in our understanding of experimental evidence places a finite limit on the ultimate elements into which this process of composing the gross out of the less gross may resolve physical bodies. The process itself is no other than the endless one whose unlimited continuance is a condition to unlimited progress from lower to higher orders of

approximation; it involves the incessant reopening of natural systems previously taken to be "sufficiently" closed. The limiting conception toward which this progressive decomposition of grosser bodies of physical imagery into less gross constituents tends, is of course the mechanical point-image, in terms of which we have been able to define that to which the approximations of higher and higher order progressively approximate.

And so, in conclusion—to adjust his observations of gross bodies to some physical image of the natural system observed; then, progressively to narrow the range of mechanical images in which point-groups may constitute the physical images of these gross bodies—this we take to be the eternal task of the physicist.

And just so, be it noted, did Hertz understand the physicist's function, and so he felt assured was that function understood by all physicists of his day.

"Alle Physiker sind einstimmig darin, dass es die Aufgabe der Physik sei, die Erscheinungen der Natur auf die einfachsten Gesetze der Mechanik zurückzuführen." Nor do his words here or elsewhere imply any suspicion on his part or on the part of his colleagues that this task had met, or was likely to meet with an obstacle which would eternally check its successful performance. The only division of opinion among the physicists of his day concerns the choice of ways in which this adjustment of physical observation to mechanical imagery was to be effected. "Welches aber diese einfachsten Gesetze sind, darüber herscht nicht mehr die gleiche Einstimmigkeit." And he states his own problem in presenting his *Prizipien* in words which reflect no doubt that in some ways the required adjustments of physics to mechanics can be made; his object is to suggest a simpler way to effect this adjustment than his predecessors had

devised "Die Aufgabe, deren Lösung die folgende Unter-
suchung anstrebt, ist diese, die hier vorhandene Lücke
auszufüllen und eine vollkommen bestimmte Zusammen-
stellung der Gesetze der Mechanik anzugeben, welche mit
dem Stande unserer heutigen Kenntnis verträglich ist,
welche nämlich in Beziehung auf den Umfang dieser
Kenntnis weder zu eng ist, noch zu weit ist "

No doubt, in all this, Hertz correctly interprets the
"unanimous" understanding of his fellow physicists of the
closing Nineteenth Century, nor does this unanimity seem
to have been disturbed anywhere in the scientific world
during the first two decades of the Twentieth Century.
Then in the year 1927 a publication appeared that split
the community of physicists into two contending camps
What that publication was, and what followed on it, is the
topic of the following chapter.[2]

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[1]H Hertz, *Die Prinzipien der Mechanik*, "Vorwort des Verfassers "
[2]Editorial Note Presumably referring to the publication of W.
Heisenberg's "Über den anschaulichen Inhalt der quantentheoretischen
Kinematik und Mechanik," *Zeits f Physik*, Vol 43, 1927 The next
chapter was to have discussed indeterminism in modern physics, but for
reasons given in the Introduction this has not been done Heisenberg's
The Physical Principles of the Quantum Theory, University of Chicago
Press, 1930, does argue that the "uncertainty relation specifies the
limits within which the particle picture can be applied," and thus
explains *his* meaning of "uncertainty " This meaning does not seem to
conflict with Singer s postulates given in Chapter 15 For a further dis
cussion of this topic, see C T Ruddick, "On the Contingency of Natural
Law," *The Monist*, 1932 (p 530)

18. The Producer-Product Relation

IF THE PHYSICIST'S ARGUMENT FOR INDETERMINISM IS historically episodic and humanly limited in the public competent to debate it, not so the biologist's, or to come at once to the general case, the "biocentric scientist's" "The general case," we say, for we recall a previous chapter to have subsumed under the generic term "biocentric sciences" a number of special studies of which biology was one Denotatively, these sciences are concerned with all that makes up the domain of life, its organs, instruments, social groupings. Connotatively, they are concerned with no bodies which the physical leaves unobserved, but they recognize in the objects of their study properties the physicist disregards, namely, the properties called *functional* Now, the attribution of functional properties to the components of the biocentric world is world-old and world-wide, and the recognition of certain difficulties in the way of adjusting objects possessing functional properties to any kind of mechanical imagery is as ancient as human reflection on matters of common experience Of these difficulties, the principal ones may be noted in an anticipatory way, at least to the extent of giving name to certain traditional worries

First, functional properties seem to be as "inherent" in the subjects possessing them as are those structural properties, configuration, volume, velocity, mass taken by the physicist to be invariant with variation of environment Yet no account need be taken of these inherent properties, as data required for the physicist's predictions and explanations

—in short, for his adjustment of observational data to a mechanical imagery between whose momentary distributions a cause-effect relation exists. How, then, adjust such observations of natural objects as recognize in some of these objects functional properties, to an imagery that has as yet no way of distinguishing the representation of an object that does from the representation of one that does not possess functional attributes?

In the second place, the constituents of a biocentric world seem to be subject to no such completely determining laws as are the points of a mechanical image, but what "norms" they do conform to allow them a certain indetermination, illustrated in the "spontaneity" of life in general, the "freedom" of man in particular. How can things exhibiting spontaneity of action, freedom of behaviour, be embodied in the details of a natural system that can be adjusted point by point to an image whose constituent points are completely devoid of functional attributes, and completely determined by law as to their changes of position and attribute?

Thus we come to a rough picture of the quandary in which the mind of the historic past has found itself entangled. Without function and freedom, how give meaning to life? With function and freedom, how conform life to mechanism? Without mechanism, how relate cause and effect? Without cause and effect, how understand anything? Throughout a long past, the mind has tried numerous devices by which to resolve its perplexities. None has received the sanction of any preponderance of thinkers; none shows evidence of growing in favor more conspicuously than its rivals. It is left for each who sees the difficult to seek his own way of extricating himself.

Now, it would suggest itself to anyone who has followed the argument of our previous chapters, that if by any means

our observation of the biocentric world is to be adjusted to the demands of mechanical imagery, it must be by resorting to one or another of the devices listed under *Post iii*, for the devices there listed are supposed to exhaust the expedients that have been successfully used in other instances to recover a lost ability to meet the demands of *Post i* The last of these expedients for re-establishing adjustment of data to imagery, where under the strain of accommodating new data to old images all previous methods of adjustment had failed, proposed as alternative possibilities either (a) a revising of the data to be adjusted, or (b) a devising of new methods of adjustment (Chapter 15 p 206 ff) And as an experiment involving both a redefining of the new data which the phenomena of the biocentric world open to our observation, and a devising of methods not needed in previous chapters for the adjustment of data to the demands of mechanical imagery, it might appeal to anyone as a promising beginning to consider this problem Whether we could not represent in a mechanical image to which a physical world had been adjusted, images of a class of bodies existing in this world, yet (a) possessing properties recognizably functional in nature, and (b) subject to only such "behavior-controlling" conditions as were rather rules of the usual than laws of the necessary?

As a first step towards justifying an affirmative answer to this question, the present study proposes to distinguish two relational terms ordinarily used quite interchangeably, the one a *cause-effect*, the other a *producer-product* relation

To begin with an example of the latter relationship, used in a sense our analysis intends to preserve—imagine the inheritor to an old estate points to a sizable oak, remarking that his oak was said to be the outgrowth of an acorn planted

by his grandfather a hundred years ago With what condi-
tions of mechanical imagery would his statement have to
conform, if it was to be accepted as true? That is, in what
kind of mechanical image of the natural system of which this
oak forms part, would one find two details, one, the image
of that acorn, the other, of this oak, such that, of the two
objects imaged, the first was to be recognized as a producer
of the second, the second, a product of the first?

Evidently, in a mechanical image of any "sufficiently
closed" natural system in which a present and a past
acorn had their respective places, the acorn would be repre-
sented in some detail of a point-distribution imaging that
natural system as it was a hundred years ago, the oak, in
some detail of distribution imaging the system as it is today
Of these two distributions, the first would be cause of the
second, the second, effect of the first Now, if between an
acorn and an oak so imaged, a producer-product relation is
to be recognized, two requirements will have to be met
under the determining law of any mechanical imagery to
which that natural system is adjustable, the presence of that
acorn image in the cause will have to be (1) a *necessary*, but
(ii) an *insufficient condition* to the presence of that oak-
image in the effect Thus, the grandson speaking in our
example would be the first to agree that his statement could
not be true, if either of two other statements were false,
the first asserting that had his ancestor not placed an acorn
in the indicated then and-there, no oak would have stood
in the indicated here-and-now, the second asserting that
though his ancestor had placed an acorn at the time and
place indicated, there might still have been no oak where
the present one now stands We are not asking, for the
moment, what evidence the speaker would have to gather
to confirm his belief that both these statements were true,

but only, what evidence would have to establish, if his original statement was to be accepted

We ask, then, what formal conditions would this mechanical image of a given natural system have to fulfil, if the acorn image in the cause and the oak image in the effect were to represent two natural objects, of which the existence of the first was a necessary but insufficient condition in the existence of the second? To arrive at an answer, the present study follows a procedure which might be called, *the method of virtual replacements* (reminiscent of the principle of *virtual displacements* familiar to the literature of analytical mechanics) A simple symbolism lets us follow the application of this method more accurately and more graphically than would an account couched entirely in verbal phrase Presenting the problem and its solution in terms of the acorn oak illustration already before us, it will be easy to generalize the method and its result to apply to any two objects of which the first is shown to be the producer of the second

The discussion preserves throughout the symbolism proposed in the last chapter (p 262) in which

  x represents structures of a mechanical class

  $\curlyvee$ represents structures of a physical class

  x represents structures of a morphological class

In expression of the form $(x_1 + \bar{x}_1)$, it is to be noted that, while the x_1 represents an individual structure of the x class, the environmental symbol $\bar{x}_1$ connotes nothing of the mechanical class to which this environing structure belongs, it merely *denotes* the region of the total point-distribution in which this environment is to be found So in any expression of the form $(x'_1 + \bar{x}_1)$, in which x' replaces x, the symbol x' connotes a definite mechanism *not* of the

x-class but the environmental symbol $\bar{x}_1$ *does not* connote a mechanical structure of a class other than that to which belonged the $\bar{x}_1$ which it replaces, nor has it any other structural connotation; it continues simply to *denote* the structure of the region by which the x'_1 structure is surrounded.

Let x_{1i} be a member of the (mechanical) class x; and $\bar{x}_{1i}$, its environment in the mechanical image of a sufficiently closed natural system at a moment t_i; then the total point distribution at that moment will be imaged as the sum

$$(x_1 + \bar{x}_1)_i$$

Analogously let y_{1j} be a member of the y-class, and $\bar{y}_{1j}$, its environment at a moment t_j; then the total point distribution of that moment will be imaged as the sum

$$(y_1 + \bar{y}_1)_j$$

If, then, of these two moments t_i and t_j, t_i is the earlier date, t_j a later, $(x_1 + \bar{x}_1)_i$ will be the cause of which $(y_1 + \bar{y}_1)_j$ is the effect.

In the mechanical image of a given natural system, let

$$(x_1 + \bar{x}_1)_i \qquad \qquad (a)$$
$$(y_1 + \bar{y}_1)_j \qquad \qquad (b)$$

represent the total point distribution of the system at the moments t_i and t_j, respectively, in which x_{1i} is a member of the (mechanical) x-class, individuated by its space-time coordinates s_1, t_i; y_{1j} is a member of the (mechanical) y-class, individuated by its coordinates s_1, t_j, and in which $\bar{x}_{1i}$ and $\bar{y}_{1j}$, respectively, image the total momentary environment of x_{1i} and y_{1j}. Since the point distribution symbolized in (a) and (b) are momentary conditions of a mechanical image of the same natural system, there must exist between them a cause-effect relation, in which (a) is cause of (b), (b) is effect of (a).

THE PRODUCER PRODUCT RELATION

Now, if the image thus symbolized is to represent a natural system in which a given acorn is to be a producer of a given oak, an individual x and y may be found, of which the x images the acorn, and y images the oak Let a symbolize the morphological class to which all acorns belong, o the morphological class to which all oaks belong The x_{i_1} must belong to both the *mechanical* class x and to the *morphological* class a, i e to a class which the formal logician represents by the symbol xa Analogously, y_{i_1} must belong both to the mechanical class y and to the morphological class o, therefore to the class yo Giving to the general symbols of (a) and (b) the special form they must take on to let them image the natural objects indicated, the two expressions become

$$(x_{i_a} + \bar{x}_i)_i \qquad (c)$$
$$(y_{i_o} + \bar{y}_i)_i \qquad (d)$$

But to make our symbolism less cumbersome, even though less perfect, let us, *without change of meaning*, write (c) and (d), respectively, in the form

$$(a_1 + \bar{a}_1)_i \qquad (e)$$
$$(o_1 + \bar{o}_1)_i \qquad (f)$$

of which (e) is the cause of (f), (f), the effect of (e)

With this, we set down as the first condition to be met by a natural system in which a given acorn is a producer of a given oak

A The natural system must be susceptible of adjustment to a mechanical image in which

$$(a_1 + \bar{a}_1)_i \text{ is the cause of } (o_1 + \bar{o}_1)_i$$

where a_{i_1} images the acorn, o_{i_1} images the oak

The remaining conditions to be met by a natural system in which a given acorn is a producer of a given oak, are much more difficult to express symbolically, for both are conditions whose verbal expression takes the form of a

hypothetical proposition in which either (i) the antecedent
or (ii) the consequent is a "condition contrary to fact"
These two hypotheticals are, respectively, (i) "if the natural
system had not contained an acorn at a given *there* and
then, it would not have contained an oak at a given *here* and
now, (ii) Though the system had contained an oak at the
given *there* and *then*, it might not have contained an oak at
the given *here* and *now* The following symbolism, making
use of a method called the method of "virtual replacement,"
undertakes to set down the two conditions requiring the
existence of an acorn *there* and *then* to be (i) a necessary,
but (ii) an insufficient condition to the existence of an oak
here and *now*

First, as to the necessity of the producer to the product
Referring to condition A, above, let the acorn-image, a_1, in
the cause, be replaced by a non-acorn image, a'_1, and let the
law of the natural system imaged determine the effect that,
resulting from this hypothetical cause, would replace the
actual effect given in A Then, the acorn imaged as a_1 in the
actual cause is a necessary condition to the oak imaged as o_1
in the actual effect, if, and only if, the following hypo-
thetical proposition holds

B If the cause had been $(a'_1 + \bar{a}_1)$, the effect would have
been $(o'_1 + \bar{o}_1)$, For this proposition shows our image to
represent a natural system in which the substitution of a
non acorn for the acorn imaged in the cause, would have
made the cause, thus distorted, to have for its effect the
image of a system in which the oak was replaced by a non-
oak

Second, as to the insufficiency of the producer to the
product Referring as before to condition A, let the oak, o_1,
imaged in the effect, be replaced by the image of a non oak,
o'_1, and let the law of the natural system imaged determine

the cause which would result in this effect Then the acorn imaged in the actual cause is *an insufficient condition* to the existence of the oak imaged in the actual effect, if, and only if, the following hypothetical proposition *does not hold*

C If the effect had been $(o'_1 + \bar{o}'_1)_i$ the cause would have been $(a'_1 + \bar{a}_1)_i$

For the failure of this proposition to hold, shows our image to represent a natural system in which the replacement of the oak imaged in the effect by a non-oak, *need not* have resulted from a cause in which the acorn imaged as a_1 was replaced by a non-acorn, i e the acorn could have been present in the actual cause, yet the oak absent from the actual effect

To establish experimentally the falsity of proposition C, is, indeed, a necessary condition to recognizing the existence of any given acorn existing in any given natural system as insufficient to assure the existence of some given oak in that system

This non-implication could find empirical support on only one ground, namely, that experience shows it possible for the "oakless" distribution of the effect symbolized in the antecedent to have been caused by some other that the "acornless" distribution symbolized in the consequent But in an image that has met our first requirement (A), the hypothetical distribution of a cause that would *satisfy both* the requirement of being other than the hypothetical one set in our nonimplication (C) and also other than the actual one represented in (A), would have to take the form $(a_1 + \overline{(a)'_1})_i$ in which $\overline{(a_1)}'$ represents a structure of some other mechanical class than that to which belongs the actual environment set forth in (A) Expressed in the idiom of virtual replacement, the third condition to which an image would have to conform, if it is to represent a natural system

in which the required acorn-producer-oak relation is to be found, will be expressed in the hypothetical proposition,

(C) For some environment of a_1 other than $\bar{a}_1$, had the cause been $(a_1 + (a)_i{}')$, the effect might have been

$$(o_1 + \bar{o}_1),$$

If, and only if, the determining law of our imagery validate this hypothetical proposition (C), will the image represent a natural system in which the presence of a given acorn in a cause is an *insufficient* condition to the presence of a given oak in an effect

Reflection on these three formal requirements whose fulfilment is necessary to constitute an image of a natural system one of which a producer-product relation exists between a given acorn and a given oak, must begin with the assumption that the image which furnishes the categorical premise (A) correctly represents the natural system in question. As this image merely affirms that there was an object of acorn-morphology at one time and place, and an object of oak-morphology at another time and place, the truth or error of this affirmation could be experimentally confirmed or refuted without raising the question of any relation between the two objects imaged other than a difference of space-time location and a difference of morphology. Had experimental evidence not confirmed the facts stated, no question as to whether or not a produce-product relation connected the imaged acorn with the imaged oak could have arisen.

Since the question has arisen and the relationship affirmed, we understand the one who so affirms to have satisfied himself on some evidence of the truth of the hypothetical proposition (B). As in confirming any hypothetical statement in which the antecedent presents a condition

contrary to fact, the evidence on which confirmation would rest must be the application of some general induction from observed facts, to the case of a particular fact of the class to which induction refers. As every one knows a field bearing a crop of wheat could not have been planted in rye, one bearing a crop of rye could not have been planted in wheat, one bearing neither could not have been planted in either. So with oak-growing and acorn-planting Oaks do not grow in spots we know to have been planted with walnuts or with pebbles To establish evidence of this inductive-deductive kind, can furnish the experimental scientist with some of his most difficult and important problems, it was on such evidence that Pasteur came to the conclusion that had there been no organisms of a certain morphology in a certain medium, there would have been no ferment in it later

But, though to have gathered evidence in support of hypothesis (B) is a necessary condition to establish a producer-product relation between our acorn and our oak, it is not a sufficient condition to the proof of such a relationship It is just as necessary to confirm the second hypothetical proposition, (C), which is our ground for saying that to have planted an acorn at the time and place specified in (A) was not enough to have assured the existence of an oak at the time and place specified in that expression By way of empirical evidence to support the hypothetical proposition (C), we can again neither offer nor ask any more convincing than the general observation that oaks grow from acorns under some conditions of space-time environment—soil, climate, accident of individual history, and do not grow under other environmental conditions Such evidence is of the same order on which the parable of the sower depended for its significance to the humanity of 2,000 years ago Its force is proportionate to the generality of the experience to

which it appeals And how forceful the support of general
experiences can be, the universal appeal of the parable
witnesses What generation of man so primitive as not to
know that seed fallen by the wayside, seed fallen upon a
rock, seed fallen among thorns, seed fallen on good ground
would have very different "life-expectancies"? And who
would not risk the expectation that what was true of all
known kinds of seed would be true of any untested kind,
or even more generally, that what was found to be true of
every known kind of "coming into being and passing away",
could be taken for true of any unproven case?

It is understood then, that the image of a natural system
which would represent a certain acorn as producer of a
certain oak must fulfil the three conditions A, B, C But are
these the only conditions such an image need fulfil? The
present study accepts the satisfaction of these three require-
ments as not only a necessary but a sufficient condition to
constitute the point-aggregate making them a mechanical
image of a natural system in which a given acorn is a
producer of a given oak Moreover, our study accepts the
three propositions that result from substituting x for a,
y for o, as formulating the requirements whose satisfaction
is a necessary and sufficient condition to constitute the
point-aggregate making them a mechanical image of a
natural system in which an individual of x-morphology is
an x producer of an individual of y-morphology But con-
siderations of pure logic connect the acceptance of any
condition whose fulfilment enables us to image, in the way
described, an x_1,- producer -y_1, with the acceptance of the
conditions whose fulfilment enables us to image the contra-
dictory relationship, x_1- non-producer -y_1, For convenience
of future reference the conditions establishing the contra-
dictory relations of *production* and *nonproduction* between a

given x and a given y will be set down in close juxtaposition
Conditions establishing the relation x_{1i}—*producer*—y_{1j}
1   For a given set of the four structures $x_{1i}, \bar{x}_{1i}, y_{1j}, \bar{y}_{1j}$
$$(x_1 + \bar{x}_1)_i \text{ is cause of } (y_1 + \bar{y}_1)_j$$
2.   For any structures of the non-x class, x'
$$(x'_1 + \bar{x}_1)_i \text{ would be cause of } (y'_1 + \bar{y}_1)_j$$
3.   For at least one environing structure of the non-x class,
x',
$$(x_1 + (\bar{x}_1')_i \text{ would be cause of } (y'_1 + \bar{y}'_1)_j$$
Referring by number to these three conditions, we say that
in a mechanical image of a natural system in which occur
objects imaged as x and y, respectively,

Definition I. x_1 represents a *producer* of y_1, y_1 represents a
product of x_1 if, and only if, the image fulfils conditions
1, 2, 3

Definition II x_1 represents a *nonproducer* of y_1 and y_1 repre-
sents a *nonproduct* of x_1, if, and only if, the image fulfils
conditions I, but *not both* 2 and 3 As the contradictory of
a conjunction of propositions is the disjunction of their con-
tradictories, the condition set down as *not both* 2 and 3 is to
be read, not 2 *or* not 3, i e *either* 2 shall not hold *or* 3 shall
not hold And this, as the previous discussion shows, is
equivalent to imposing on the image the conditions that the
x_1 appearing in the cause shall be either (not 2) an *unneces-
sary*, or (not 3) a *sufficient* condition to the appearance of y_1
in the effect Illustrations of the way in which these condi-
tions would be realized in experience will be offered in the
course of a commentary to follow

Two more definitions complete the list of those presenting
such terms relating to *actual production* and *actual non-
production* as will be needed for the discussion in the next
chapter of *possible production* and *possible nonproduction*,
with its subordinated species

Definition III The manifold of *x-producers-y* that will have
existed in a given natural system by a given date, consists of
all individuals of the *x*-class belonging to that system of
which each, by that date, will have fulfilled, with respect to
some individual of the *y*-class, conditions (1) (2) (3),
imposed by Definition I on an *x*-producer-*y*

Definition IV The manifold of *x-nonproducers-y* that will
have existed in a given natural system by a given date,
consists of all members of the *x*-class of which each, by that
date, will have fulfilled with respect to *all* individuals of the
y class of past or future date conditions I, and not 2 or not 3
imposed by Definition I on an *x*-nonproducer-*y*

On the definition of the producer-product relation and its
contradictory will depend the definitions of such other terms
as will ultimately let us formulate the meaning of *function*
Since this is so, it will be well to interrupt the development
of our theme long enough to consider certain implications
of our definitions that may not be immediately apparent,
and certain apparent implications that will raise critical
questions

Commentary

(a) In the first two definitions, the only point that sug-
gests a need of explanatory comment concerns the disjunc-
tion, not 2 or not 3, imposed by Definition II on an *x*-
nonproducer-*y* Together they require that a given *x*
standing in this relation to a given *y* shall be either (not 2)
unnecessary or (not 3) sufficient to the existence of the *y*,
under conditions formulated in I How is this disjunction
of conditions realized in experience?

Generally, of course, the first alternative is the one
empirically established in judging a given acorn (say) to
have been *not* the acorn that produced a given oak It is easy

to conclude that an acorn planted in some remote region of the earth could not have been the one that produced the oak standing here The impossibility here accepted is to be sure not of a definitional, but of an empirical order, it is of the kind commonly called a "practical impossibility", which does not exclude the "bare impossibility" of an acorn planted however far away from a y here present having been transported by a most unlikely accident to the "here" in question But on the acceptance of such "practical impossibilities" excluding from consideration the "bare possibilities" theory admits, rests our whole system of empirical science

But though in practice, the disjunctive condition "either unnecessary or sufficient" is generally satisfied when evidence supports the first alternative, the critical mind is bound to ask itself whether fulfilment of the second is left without illustration in the whole world of experience Though our affairs of the market place may never depend for their outcome on our ability to recognize nonproducers that are so because they are both necessary and sufficient to the existence of some result which might mistakenly be taken for their product, the recognition of this condition of nonproduction has at least an analytic importance It excludes the possibility of anything in the world being both a producer and a cause of anything else therein The question might arise in some such connection as the following It is easy to see from all the definitions so far offered that, in a mechanical image of a natural system, the cause-distribution of an earlier moment is a necessary and sufficient condition to the effect-distribution of a later moment, while any detail of the cause is necessary but not sufficient to the total effect distribution If it should have occurred to one to ask himself whether the cause-effect relation

might not be regarded as a special "limiting" case of a
producer-product relation, the answer is provided by this
condition, not 3, controlling producer-product relation-
ships cause has to be, the producer cannot be both a
necessary and a sufficient condition to the effect

(b) The preceding episodic use of a formal definition to
distinguish cause-effect from producer-product relation, is
but an incident in a systematic comparison of the two
relationships, such as the formal logician would consider
necessary to establish the adequacy of definitions to differen-
tiate terms to which different names have been given This
systematic comparison of definitions is effected by attribut-
ing to relationships certain "formal properties", of which a
few will serve us here to test the adequacy of our definitions
of the *cause effect* and *producer-product* relations These
few suffice to establish at least one formal difference, in the
presence of several important resemblances, between two
terms ordinarily treated as synonymous, but here sharply
differentiated

To recall the logician's definition of the few formal pro
perties that need here be considered by way of comparing
these two relationships, let a, b, c   represent terms
related, and r_1, r_2, r_3 relationships between terms
Then the relation r is   *if and only if*
reflexive   a r a
symmetrical   (a r b) implies (b r a)
transitive   (a r b) and (b r c) implies
                                     (a r c)

The asymmetric relations r_1, r_2 are   if and only if
mutually reciprocal   (a r_1 b) implies (b r_2 a)
                                          and
                                (b r_2 a) implies (a r_1 b)

In terms of the formal properties thus defined

the *cause-effect-relation* is	the *producer-product relation* is
reflexive	non-reflexive
asymmetric	asymmetric
transitive	transitive
the relations *cause-effect* and *effect-cause* are mutually reciprocal	the relations *producer-product* and *product-producer* are mutually reciprocal

So classified, the two relationships are seen to share all formal properties, save one *cause-effect is, producer-product is not* a reflexive relation

The reflexive property was conferred upon the cause-effect relation when in first defining it (supra, p 198), the date of an effect, t, was made either later than or identical with the date of its cause, t. It was intimated at the time that an element of choice was involved in accepting the adjective *identical* in thus fixing the time relationship between the two dates, a choice that establishes cause effect a reflexive relation Is it similarly a matter of choice as to whether we shall or shall not make *producer-product* reflexive? Does it lie within our choice to impose upon these two relationships whichever we prefer of the four possible combinations making (1) neither, (2) both, (3) one and not the other, or (4) the other and not the one, reflexive? To consider all these cases would require closer analysis and introduce more subtleties, than the importance of some of the questions would warrant But the combinations most likely to present themselves to the mind of the formal scientist as deserving special consideration can be examined as to whether the decision is optional, and if so, how the option is to be exercised

That it would be a simple matter so to define the two relationships as to make neither reflexive, yet leave both,

as redefined, of undiminished usefulness to empirical
science, is easily seen We have only to require that the date
of an effect be later than, never identical with that of its
cause Since the definition of producer-product introduces
Condition 1, requiring a cause-effect relation to exist be-
tween the distributions in one of which a producer, in the
other a product is to be a detail, the ban against recognizing
this relation as existing between a distribution and itself
would make it impossible to define any relation other than
that of logical identity between a producer and itself And
doubtless this choice has something to recommend it to
"common sense", with that organ's rooted and not un-
motivated distrust of reflexive categories—a distrust one
recognizes in spite of the ease with which common sense
refers to one who is "not himself", "beside himself", "above
himself", or on the other hand comes to be "himself again "

What then of the alternative arrangement (2) which
would make both the cause-effect and the producer-product
relations reflexive? This, too, would find something to
recommend it to one accustomed to use the two relations
interchangeably But that it would not be possible to accept
this alternative is seen, when, to test it, we identify the x
and y, the i and j, imposed by Condition 3 on the cause-
effect relation This condition would then read

$$(x_1 + \bar{x}_1')_i \text{ would be cause of } (x'_1 + \bar{x}'_1)_i$$

But, if the left-hand member of this expression is to be a
cause of itself, one should have to accept it, as the cause-
distribution of identical effect-distributions, both

$$(x'_1 + \bar{x}'_1) \text{ and } (\overline{x_1})')$$

This would be consistent only if these two expressions were
different ways of symbolizing one and the same distribution
But as the two expressions represent one and the same
region of distribution, they respectively symbolize this

region as occupied by structures of the contradictory classes x' and x As it is impossible to suppose these two distributions identical, it is impossible to make both the cause-effect and the producer-product relation reflexive For the rest, it would be sufficiently difficult to imagine any sense in which a producer whose presence in a cause is an insufficient condition to the appearance of its product in any *later* effect, could be no less insufficient to its appearance in an effect identical with itself as cause

As to making the producer product relation reflexive, the cause effect, not, the suggestion would be *too arbitrary and purposeless to recommend itself to any one, or if it did, would be dismissed as awkwardly difficult of expression* This leaves only two combinations open to our choice to make both relations non reflexive or to make cause effect reflexive, producer-product, not Of these alternative possibilities, our choice has fallen on the latter, for reasons that are not difficult to explain

In the first place, these reasons *do not derive from any general, and supposedly logical, necessity of regarding everything as cause of itself* If that were true, we should have to recognize every detail of a momentary point-distribution in any mechanical image as cause of itself But, by definition, the cause effect distribution can only exist between the two momentary distributions of a closed system, and in any such system some if not all details of momentary distributions *are not those of a closed system* On the other hand, if a cause effect relation exists between two distributions of a mechanical image, of dates t_i and t_j, respectively, then the like relation exists between two distributions separated in time by an interval less than any given interval Now, no principle of mathematics requires that a condition holding of a function as it approaches its

limit to within a distance less than any given distance shall
also be taken to hold at the limit; there are many functions
of which this is demonstrably untrue. Nevertheless, where
there is no reason why it should not be taken to hold, it is
generally preferred by mathematicians to define it as
holding. So here, as there is no reason to prevent a relation
that holds between any two distributions of a mechanical
image as the interval between them approaches the limiting
value zero, from holding at the limit, there is strong reason
for accepting it as so holding. This we have done,
making the cause-effect relation reflexive; and as it has
already been shown why we could not so define the cause-
effect and the producer-product relations as to make them
both reflexive, the reason stands explained for the formal
properties attributed to these relations in our foregoing
comparison of them. With this systematic classification of
the formal properties of the cause-effect and producer-
product relations, there is no danger, in spite of the many
such properties they share, of our ever being tempted to
suppose, under all circumstances, however special, that
two terms could stand simultaneously in both relations the
one to another. A reflexive relation can never be a non-
reflexive relation.

(c) The producer-product relation connecting the germs of
life with its more mature forms is not the only such relation
of importance to the biocentric sciences. Ecology, studying
the conditions of environment more or less favorable is a
specific productivity of the object environed, is best known
to and more highly developed by the science for which it
has the greatest practical importance: biology. But it is no
less a matter of major concern to the biocentric sciences of
technology, economics and many others. To discuss the
meaning of environmental conditions "more or less

favorable" to specified types of production would involve
categories of *profitable production* not yet defined But we
need not wait for these definitions to have been formalized,
before considering a limiting case with which all the others
have much in common, the case in which all environments
are classed as either *productive* or *nonproductive* of specific
products The ultimate classification of all environments in
terms of their greater or less likelihood of being productive
can wait later developments

In most, if not in all types of production, the distinction
between productive and (absolutely) nonproductive regions
involves some physical conditions present in the one class,
absent in the other The soil and climate that grow wheat
will not grow rice, the habitat of the polar bear cannot be
shared by the chimpanzee Since then the physical condi-
tions that permit one and not another kind of growth vary
only within certain limits, all structures falling under the
head of an environment either productive or nonproductive
of a given type of product constitute what we have called a
morphological class And, as the environed mechanism
of a mechanical class X and of a morphological class X, has
been, in our economized formulas, symbolized as $\bar{x}_1$, so the
environment $\bar{x}_1$, if of the morphological class $\bar{x}$, may be
symbolized as x In this case, two interesting results follow
the conditions (1) (2) (3) on which depend our definitions of
producer and *nonproducer* are susceptible of greatly simpli-
fied expression, and, so expressed, the productive environ-
ment will be found to fulfil exactly the same requirements
as those imposed upon the productive structure environed
That is to say, under the conditions specified, the structure
and its environment are *co producers* of a given product
The range of cases in which the environment is limited to a
well defined morphological class, is sufficiently wide, and of

sufficient practical importance to the ecologist, to make the analysis of these results worth preserving for future reference. As, however, this range cannot without careful examination be assured complete generality, and is, in any case, not indispensable to our further developments, the text need not be burdened with an analysis which may more conveniently be committed to an appended note at the end of this chapter.

(d) It will have been noticed that in the definition of the manifold, *x-actual-nonproducer-y*, the conditions imposed upon any x to be included in this manifold at any given date, require it to be recognized as not merely an x that had *not yet* produced a y, but as an x that *never will* produce a y. If it be asked how observation at a given date can have established a provision that relates to an endless future, one can point to a single example from which it is easy to generalize a rule applying to all. An acorn, observed to have been consumed by fire, is accepted as an acorn that never will produce an oak. The observation made, this acorn is to be included in the manifold of acorns that are actual nonproducers of oaks.

(e) It will have been noticed, too, that the definitions of both actual producers and actual nonproducers refer to the collectivity of structures recognized to be one or the other as constituting a manifold, not as constituting a "class". Whether or not these manifolds are in any sense to be called classes, and if so, in what sense, is a question left to the consideration of the next chapter, where our development will pass from the definition of *actual* production and nonproduction, to the definition of *possible* production and nonproduction together with such other "modal" categories of production as depend on the possible for their meaning. It is to be expected that in the course of these later develop-

ments, the draughts made on "general experience", explanatory of conditions imposed on a mechanical image in which the representation of producers and nonproducers was to be recognized, will be replaced by explicit premises, i e certain formal conditions to which our observations of Nature are to be adjusted, if a natural system in which empirical science assumes certain relationships to exist is to conform with the requirements of mechanism

APPENDIX

1 In symbolizing the simplified conditions introduced with the provision that the environment, as well as the environed producer, belongs to some morphological class symbolized X, it will be sufficient to consider only the changes this provision makes possible in the expressions of the causal distributions, 1, 2, 3 These distributions may now be written

1a $(x_1 + \bar{x}_1)_i$,

2a $(x'_1 + \bar{x}_1)_i$

3a $(x_1 + (\bar{x})'_1)_i$,

Since, in 2a and 3a the condition now holds for all structures falling within the x and the $\bar{X}'$ classes, respectively, it is no longer necessary to introduce the verbal distinction between 2 and 3, making the former hold for *all*, the latter for *some* structures of the variable to which they respectively, refer

The application of a familiar principle brings us to the conclusion that, under the provision that the environment of x_1 is confined to the morphological class $\bar{X}$, the environment and the structure environed are co-producer of the product y_1, namely, since any detail in a momentary point distribution is the environment of its own environment, the x_1 of the distribution in 1a, 2a, 3a may be written $\bar{x}_1$ So writing our three conditions, commuting the summanda in

each, and transposing the order 2a, 3a, to the order 3a, 2a, the three conditions may be written in the form

   1a $(\bar{x}_1 + \bar{\bar{x}}_1)$,
   3a $(\bar{x}_1 + \bar{x}_1)_i$
   2a $(\bar{x}_1 + \bar{x}'_1)_i$

It will be seen that these two ways of saying the same thing impose identical conditions, the one, on a structure x_1, the other, on a structure $\bar{x}_1$, whose fulfilment would make of either structure the image of a producer of y_1 That is, the structure and its environment are co-producers of the product in question, a result that is not surprising, considering that of any structure and its environment, each is the environment of the other

19. The Modal Categories of Production

IN ITS INTRODUCTION AND INTERPRETATION OF THE producer-product relation, the study of the last chapter was conducted in tacit acceptance of a principle which, when formulated, will be recognized as partly familiar, partly novel Familiar, in that the need to which this principle is science' formal response has already been recognized as implicitly felt and actively met by nonfunctional sciences whose historical development was followed in Chapters 15 and 16 New to the present discussion will be the extension of this principle to meet the needs of the functional sciences whose understanding and experimental method is to be the topic of our reflections throughout the remainder of this work This need, seen to be present to some and expected to motivate all sciences struggling to base their acceptances and rejections on experimental evidence alone, has already been worded To organize whatever an experimental science may accept as the "ultimate data" of its observation into a Nature image of any sort, that science must contribute a form of its own making to accommodate data of its own finding

That these "a priori forms", with which science must equip itself as instruments of its thought, are far from being the "a priori truths" for which Kant mistook them, we have already seen For us, they will remain to the end what they have been for the beginning schemata, no one of which is available for scientific use in the construction of a Nature image, unless the "ultimate data" of observation can be

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1a. $(\overline{x}_1 + \overline{\overline{x}}_1)$,
3a. $(\overline{x}_1 + \overline{\overline{x}}_1)_i$
2a. $(\overline{x}_1 + \overline{x}'_1)$,

It will be seen that these two ways of saying the same thing impose identical conditions, the one, on a structure x_1, the other, on a structure $\overline{x}_1$, whose fulfilment would make of either structure the image of a producer of y_1. That is, the structure and its environment are co-producers of the product in question, a result that is not surprising, considering that of any structure and its environment, each is the environment of the other.

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adjusted to it In the construction of a physical world-image, the schema contributed by science took the form of a mechanical image, the only kind of schema that could yield a definition of the cause effect relation Only when we had convinced ourselves that the observations of the physicist presented no obstacle to an adjustment of his observational data to requirements of mechanical imagery, could we design a scheme to which must be adjusted any physical image of a natural system What remains to be done, if the conception of experimental method realized in the physical sciences is to be recognized in the biocentric disciplines, is clear enough We must construct a schema, such that (a) all observations taken to establish the bodies observed as possessed of functional properties must be adjustable to it, and such that (b) the schema itself must be susceptible of representation in a physical, and therefore in a mechanical image of the natural system in which the bodies are found

   As a first requirement to the designing of such an image, the last chapter proceeded on the assumption that only an image of a natural system in which could be represented bodies standing in the relation of producer to product could meet the further requirements of a pattern in which could be represented bodies possessing the *modal* properties of production to be introduced in the present chapter And, of course, back of this thought, lies the further hypothesis that only bodies possessing these modal (among which the functional) properties of production could possess such other properties as would constitute them components of a bio-centric image of a natural system We turn, in due course, to the formulation of these postulates whose demands are to be met by a schema to which must be adjusted such observational data as are taken to establish in certain

physical bodies the possession of these modal properties of production

But, first, to return to the opening sentences of the present chapter, it recognizes that in its introduction and development of the categories of production, our study had "tacitly" contributed a formal schema to its picture of a natural system in which structures standing in the relation of producer to product were to be found. It would have been truer to say that its contribution in the way of form had been recognized under a phrase couched in the vernacular. The draught made of "general experience" was no other than a rough adjustment of familiar observations to the *formal conditions imposed by definition on structures stand-ing in the producer-product relation*. Our definition stipulated that if a certain acorn was to be accepted as a producer of a certain oak, then the "being" of that acorn must be *established a necessary but insufficient condition to the* "being" of this oak. To accept these conditions as established for any given acorn and any given oak, one would have to prove the truth of two hypothetical propositions

(1) If that acorn had not been, then this oak would not have been,

(2) though this oak had not been, yet that acorn might have been

In both these hypotheticals, the antecedent is a "condition contrary to fact", and reflection has long been aware of the impossibility of establishing, by "general experience" or scientific experiment restricted to only so much of a natural system as would hold the story of that acorn developing into this oak, the truth of either of the two hypotheticals in question

What then is an experimental science to do, if it finds that a large domain of the problems it must set itself to solve

cannot be so much as stated without acting on the assumption that hypotheticals of this order are true propositions?

The answer allowed to stand in the last chapter drew on the evidence of general experience to confirm the truth of the two hypotheticals which would make a certain x producer of a certain y. But what is this "general experience," that its evidence could establish even so much as the probable truth of the propositions in question? Is it not such experience as the acceptor of these hypotheticals could gather of the "world in general," i e the largest world he knows of wherein this x and that y have their respective beings? Let us not repeat, still less elaborate, what has already been said concerning the lesson to be drawn from the "parable of the sower" Instead, let us accept the general principle, that a world in which hypotheticals whose antecedents are conditions contrary to fact could be accepted as either true or false, is a world established by *postulating* certain premises In other words, it is a formal schema of which our science can make use insofar, and only insofar, as it can adjust its ever-increasing wealth of observations to that schema In the instance of the sower, an adjustment acceptable both to teacher and learner was readily made So long as the conclusions to be drawn from an antecedent contrary to fact in a given case were consistent with the consequences seen to follow in other cases wherein like conditions were *not* contrary to fact, teacher and learner felt their world experience furnished ample ground for accepting the hypotheticals in question as true propositions, and acting on them accordingly

To meet our present need, let construction proceed from the very simple and limited formal image of a world in which certain x's were represented as producers of certain y's, to an image in which the producing x's and produced

y's meet such new and more specific conditions as our postulating shall impose upon them. The order in which these new conditions are to be imposed, will, it is hoped, bring us progressively nearer to an image of a world adjustment to mechanical imagery, in which living things shall have their part

The postulates introducing the adjectival qualifiers of production here to be considered are so few and simple that they may be presented without interruption, leaving what may be needed in the way of explanation and illustration for subsequent comment

Postulates

I. Let there be an x_i- producer -y_i, where t_i is a given date, then, on and after the date t_i, x_i, will be *an x-actual producer-y* [1]

II. Let there be at least one x-actual producer-y, and at least one x-actual nonproducer-y, then, every x that is neither an actual producer-y nor an actual nonproducer-y will be an *x-possible producer-y*

III Let there be more than one x-actual producer-y, and at least one x-actual nonproducer-y, then every x -possible producer-y will be an *x-potential producer-y*.

IV. Let there be *at least two classes of potential producer*, u-potential producer-v, and x-potential producer-y, such that (1) u and x will be *morphologically incompatible*, and (2) v and y however they may differ in morphology, will have in common a *certain property* ρ, then (a) u and x will be two species of the genus *functional ρ-producers*, and (b) to the same genus will belong any third class of potential ρ-producers related to X and Y as these classes are related to each other

V Let there be a finite number k, of x-actual producers-y,

and a finite number n-k, of x-actual nonproducers-y, then
every x-possible producer-y will be an *x-probable producer-
y*, with a probability lying within the range k/n ± p of
becoming an actual producer-y, where p is in the nature of
a probable error, later to be discussed

Comment

(a) *Actual and possible production* Of the adjectives appear-
ing in these postulates, at least two, *actual* and *possible*, would
have been included in any scholastic list of adjectives dif-
ferentiating the categories that scholastic tradition called
modal categories of being There was and is convenience
in having some single heading under which to list adjectives
that have in common the function of differentiating the
ways in which the *being* of any subject falling under any
one of these categories stands related to the *being* of a sub-
ject falling under any other In entitling the present chapter
modal categories of production, we retain for our convenience
a caption which etymology approves and use has made
familiar But while we thus accept for, and adapt to, our
own use a caption that includes under it all the foregoing
"ways of being", this does not mean that we accept without
question an inference most historic schools drew from the
acceptance of the common name *categories* for all these
modal forms of speech, the inference, namely, that they are
names of so many different *properties* differentiating, one
from another, the subjects to which they severally apply

   The question to which this assumption gives rise can be
presented and discussed for all the categories in terms of
but two of their number, *actual*, *possible*, and whatever
argument may resolve the issue for these two, must resolve
it for all the rest In the present context the substantive
which these adjectives qualify is *production*, and the subjects

to be classified as actual or possible producers are physical bodies, already known in terms of their structural properties To give the matter at issue the clearest possible wording, let us put the question in this way Are the two manifolds of structure, *x actual producer-y*, *x-possible producer-y*, *two classes* of structure? Do the two names connote two *properties, actual, possible productivity*, differentiating the manifolds denoted by these names? Let this question be the topic of the first of our comments of the definitions offered

On considering the question asked, one's first inclination may be to answer with a ready affirmative Is not each of the two x-qualifiers, *actual producer-y*, *possible producer-y*, to be used as predicate of a sentence which says the same thing of all members of the manifold denoted by the subject, while the two say different things of the manifolds denoted by their respective subjects? What, then, can the two manifolds be, if not two classes differentiated by their incompatible properties?

But when one stops to think Does the fact related of a certain acorn, that on or before a certain date it has actually produced an oak, connote a property of that structure, beyond any connoted by its morphological description? One remembers that long ago Kant dismissed the "ontological argument" of the Rationalists, on the ground that, as there was "no describable difference between a hundred actual and a hundred possible dollars", so there could be no difference in the defining properties of an "existent" and a "nonexistent" God And he extended to all classes of object a principle which he worded in his own emphatic way, "Sein ist me Predikat" And, when one comes to think of it, is he not right? It is true, that of all members of a manifold denoted by a class name connotating a common property,

we may "say the same thing " Of the manifold of (current) phonograph discs, we may say, "All such discs are circular in shape " But is the converse of this general proposition true, must every manifold of objects denoted by whatever term constitutes a class, have a common property connoted by that term? In a current telephone directory, all the Smiths are gathered into pp 861–869, all the Joneses into pp 455–457, and never the twain shall meet In a botanical world-history, the manifold of *acorn-actual producer-oak*, and the manifold of *acorn-possible producer-oak*, as of the date 12 p m January 1, A D 1900, can have no common member, since the distinction between the two lies in a *fact of history*, at just that stroke of the clock the former manifold held only such acorns as had, the latter, only such as had not produced an oak But does the close neighborhood of the Smiths in the one part of a directory, and of the Joneses in another, connote a *Smithness* common to one group of neighbours, a *Joneness* common to the other, in such wise as to differentiate by their incompatible proper-ties the class *Smith* from the class *Jones*? No more, then, can the fact of history that on or before a given date one mani-fold of acorns had produced, another manifold had not pro-duced confer a *property of actuality*, upon the first, of *non-actuality* upon the second of these two manifolds, constitut-ing them into two classes of acorn differentiated by their incompatible connotations

True, custom sanctions the use of the term *class* in certain cases in which the distinction of one such *"class"* from another is effected by space time coordinates, not by differentiating properties A country mobilizing for war, may have called to its colors the "class of 1938" before the "class of 1937". No doubt this distinction effected by dates, does, *per accidens*, imply a difference in age, but the "class"

appelations used distinguish two manifolds in terms of date, they do not differentiate two classes in terms of age

What, then, shall we say, in reference to the Kantian contention that the *actual* and the *possible* do not differentiate two species of a common *genus* (as here, the genus *producers*), but distinguish two mutually exclusive constituents of an inclusive manifold? All interests and customs would seem to be conserved, if we agree that some (so-called) "classes" are given *in denotation*, other (properly called) classes, in connotation All the Smiths and all the Joneses would be examples of the former kind of "classification." All the acorn-actual producers-oaks and all the acorn-nonactual producers-oak, on or before a given date, would seem to be an analogous case of "denotative classification" Facts of history keep the two manifolds distinct, no incompatibility of property makes the two "classes" different In both cases, in all cases, of denotative classification, the manifolds denoted by different class names are *distinguished* by some nonidentity of their individuating space-time coordinates, they are not *differentiated* by any incompatibility of the properties connoted by their respective class-names

But if the nonidentity of their respective space time coordinates is as sufficient as it is necessary to distinguish the manifolds denoted by the terms, x-actual, x-nonactual producers-y, is it either necessary or sufficient to constitute the two manifolds, *x-possible, x non-possible producers-y*, mutually exclusive and together exhaustive?

It would be both, if the two relations, *actual producer, possible producer*, existing between an x and a y were offered as contradictory terms, but, of course, they are not If they were, all nonactual producers would be possible producers, and we know them not to be We do not take an acorn that, up to the moment, has produced *neither a bird nor a moon* to

be a possible bird- or moon-producer, we *do* take it to be a possible oak-producer. Why that should be, has been made explicit in the conditions imposed by Postulate II on a *possible producer* to be a possible nonproducer an acorn exists in a natural system in which it coexists with at least two other acorns, of which, by a certain date, the one has produced, the other, neither has produced, nor is expected ever to produce an oak, whereas, it does not coexist with an acorn actual-producer of either bird or moon The question then is as to whether the manifold, x-possible producer-y is, like the manifold, x-actual producer-y, given only in denotation, or whether in meeting the condition laid upon it in our definition, it has acquired a connotation If so, the question that gave rise to the immediate discussion is answered of the two modal categories, *actuality of production, possibility of production,* the former does not, the latter does, add an attribute to those already possessed by the structure to which it is attached.

But, looking back on the conditions imposed on *actual* and *possible* producers, do we find reason why the latter should, as the former does not, acquire a connotation? Does not the distinction between the possible and the nonpossible, no less than that between the actual and the nonactual producer, turn on a question of history? The x-possible producer-y is any x that is neither an actual producer- nor nonproducer-y, if and only if that x coexists in a natural system with at least one x-actual producer- and at least one x-actual nonproducer-y Here, it is to be remembered that throughout the present study, coexistent things are not restricted to those of which each is to be found in the momentary *space*-environment of each other, but include all things of which each is present in the *space-time* environment of each other In the light of this understanding,

compare these two questions On or before a given date in
the history of a natural system

(1) had or had not a given x produced a y?

(2) had there or had there not been at least one x-actual
producer-, and at least one x-actual nonproducer-y?

Are not both questions to be answered and only to be
answered in terms of the facts of history? Obviously they are
But let us not be hasty in drawing conclusions from this
premise If these two questions are, indeed, both questions
of historic fact, yet the facts in question concern the histories
of two very different things, the one, an individual thing in
Nature, the other, the Nature in which this and every other
thing has part To determine *whether by a given date a
given x has or has not produced a y, one need consider the
history of no other individual than the x in question* To
determine whether at a given date a given x is or is not a
possible producer-y, one has to be informed by the history
by the whole *group* of things constituting the natural
system in which this x is a member But we have seen
throughout how common, if not universal, is the result of
recognizing a thing whose properties had hitherto been
determined only by its membership in a *class*, to be also
member of a previously *unconsidered group* the result,
namely, that the thing thus newly grouped gains a new
property, connoted by a new differentiating name Examples
previously offered began with the familiar case in which a
member of the class, "good men and true," on being made
member of a certain group of twelve such men, gains the
property and takes on the function of being a juryman
From this, illustration might range through the connotation
of such terms as *congressman, citizen, patriot,* on to the very
general class, *anything in Nature,* since the very structural
properties that such a thing must have are such as no thing

could enjoy, save by virtue of its membership in a certain closed natural system, a system, namely which a given school of physics takes to be adjustable to such formal conditions as that school has imposed upon its pattern of mechanical imagery

Now, no doubt, both an acorn-actual producer-oak, and an acorn-possible producer-oak will already have been made members of the same natural system as that whose history has to be examined to determine whether a given acorn is or is not a possible producer-oak, else neither acorn would have been accorded the properties in terms of which the morphological class *acorn-structures* is to be defined But to determine whether or not a given acorn is an actual oak-producer demands no information concerning that system as a whole, not already gathered in attributing to it an acorn structure, whereas this information would not suffice to determine whether an acorn-structure is or is not a possible oak producer It is not, then, membership in a group of new individuals which confers on an acorn the property of being a possible oak-producer, but membership in a previously recognized but newly characterized group, i e the closed natural system to which all acorns belong

Such, then, are the reasons that would lead one to refuse to the adjective *actual*, and to accord to the adjective *possible* the function of serving so to qualify the substantive *producer* as to make the modal category *possible producer-y* a differentiating property of any x to which it was attached

But if the manifold of x-possible producers-y is a class, marked off from other classes by possession of a differentiating property, to which Class of classes does this class, and to which Class of properties does this property belong to the *structural*, or the *non-structural* Class?

As, in answering this question, we run into the first of the

two historic difficulties mentioned at the outset (Chapter 18, p 273) as having beset the mind of the past, *struggling to adjust structures possessing modal properties of production to the demands of mechanical imagery,* it will be well to examine with care the nature of this difficulty (To the second of the historic difficulties, mentioned in this connection, we shall return later)

As has been said, any argument that would decide the issue for the class *possible producer* would decide it for the classes *potential* and *functional producers* as well, since these are but species of the genus *possible producer* (Defs III, IV, supra) But as the ultimate interest of the biocentrist is in adjusting the world of functional bodies to the demands of a physical and therefore mechanical world order, the historic discussion of the problem before us is more commonly couched in terms of the category of richer connotation, *functional production,* than in terms of the category of simpler connotation, *possible production* For this reason, it will be well to let history introduce in terms of the complex, a question ultimately to be settled in terms of the simpler

The general nature of the problem here involved has already been announced (Chapter 18, p 275) How is the biocentrist to adjust a structure *having functional properties* to the very same mechanical image of a natural system as *that to which the physicist, ignoring all such properties,* has previously adjusted the same structure? The embarrassment of the biocentrist, faced with the need of effecting this adjustment, will be most acutely felt should the functional properties, which the physicist does but the biocentrist cannot ignore, turn out to be structural For the structural properties recognized by the physicist are taken to be the *only* properties whose magnitude need to be known for every body in the physical distribution of a moment t,

if the physical image of bodies so structured is to be adjusted to a mechanical image of a natural system in which a mutually determining cause-effect relation connects the point-distributions of any two moments, t_i, t_j. If, then, the functional properties of these same bodies turn out to be structural properties, over and above those which the physicist found sufficient to establish a cause-effect relation between two momentary distributions of a natural system, what are we to think? Must not our whole conception of the conditions determining the nature and limiting the number of structural properties a body can have go a glimmering?

But how escape the recognition of functional properties as structural in nature? For is it not generally conceded that a body to which a functional property has once been attributed will have possessed that property from the beginning and will continue to possess it to the end of the entire period throughout which its physical structure shall have conformed with the morphological conditions, imposed by the taxonomist of one or another biocentric science, on the class of objects of like name with the body in question? But in the course of its history covering this period, any individual object will have found itself in many environments. If, then, its retention of a given functional property is unaffected by any variation of environment it may have experienced, does not this property meet the one condition imposed upon structural properties? Is not this functional property "invariant with variation of environment," and if so, must it not be classed under the head of structural properties?

This reasoning, while sound in the premises that it accepts, is unsound in its neglect of others which, when included in the grounds of our reasoning, may reverse the conclusion arrived at. In the first place, the property of functional production, together with the other modal

properties (possible, potential production) whose possession is presupposed by that of the functional production, unlike the structural properties of physical or mechanical order, is unquantified One structure does not have a greater or less possibility, potentiality, function of *producing than another*,[2] as it can have greater or less mass, volume, velocity than another. Such "inherence" and independence of environment as the preceding argument would show functional properties to enjoy, is an invariance of possession, not of degree, since the properties in question have no magnitude in which they could vary, whereas in all the nonfunctional (mechanical and physical) attributes classified under the heads, *structural, nonstructural* (p 261) it was precisely their magnitude whose invariance or variance with environment differentiated the structural from the nonstructural Except as applicable to quantified attributes, the distinction between the structural and the nonstructural has as yet been accorded no meaning by the present study For the mere possession of its physical attributes, whether structural, or nonstructural, a body is no less independent of its environment than the preceding argument would show it to be for the possession of its functional attributes In a classic gravitational system, for example, no material point can lose all mass, velocity, acceleration, etc (Under proper conditions, its velocity, acceleration, etc may, indeed, fall to zero, but to have zero velocity, acceleration etc is quite a different thing from having no velocity, acceleration etc)

But is the argument so presented conclusive? Only an overhasty reading of the premises set forth in support of attributing to functional properties such "inherence" as would justify their subsumption under the head of structural properties, could have overlooked an implication of the conditional clause introducing this premise The premise

was, as we acknowledged, neither wrongly nor misleadingly stated. *If* (it said) a functional property has once been attributed to a structure, *then*, etc. But what conditions must we have found fulfilled *before* we can attribute to an x-structure the property of being a functional y-producer?

At this point, our argument may return from the historically more discussed category of functional production to the logically simpler one of possible production; for, as was forecast at the beginning, the classification of functional properties is determined as soon as that of possible production is determined. Of this, the evidence is all before us; we have but to recall Postulates III and IV to see (IV) that only potential producers of something can be functional producers of anything and (III) only possible producers of a given type of structure can be potential producers of such a structure. So that, to have "once attributed" a functional property to that kind of structure, is to have previously recognized it as a possible producer of some class of structure; and therefore, to whichever Class of classes, structural or nonstructural, the class *x-possible producer-y* belongs, to that Class must belong the class x-functional producer-ρ. But can there be any further doubt as to the Classification of classes of possible producer, or (since the two are mutually interdependent) of the property of possible production? To possess the property of possible productivity, a structure is entirely dependent on the coexistence with it in one and the same natural system of at least two other structures of the same morphological class with itself, on which one has, the other neither has nor ever will have produced a structure of some specified morphological class. Greater dependence on environment than this, it would be difficult to conceive; and if the historic mind has sometimes been tempted to confuse the "inherence" of functional, with the

invariance of structural properties, this is only to be explained on the ground of that mind's restricted understanding of "environment" A body with the structure of a fiddle will, indeed, retain the function of a fiddle, wherever in the world its history may carry it, but the body cannot have acquired that function in any but a world whose own history has carried it through moments in which some instrument of that structure had performed, and some had not and would not perform The environment which the modern mind accords to anything in Nature is not a space-environment only, it is a space-time environment

(b) *Potential and functional production* If the definition (III) of *potential producer* calls for any comment at all, it can only be in response to a question, not as to the meaning, but as to the need of such a category Could one not (it may be asked) proceed directly from the definition of *possible* to that of *functional production* without interpolating between the two the concept of *potential production*? The question is pertinent enough, and will be given consideration in a later paragraph For the moment, it is more important to clarify by illustration the conditions imposed by Postulate *IV* on the more complex concept of *functional production*, and to meet certain questions that may be raised as to the consistency of the definition offered with the meaning implied by the accepted use of terms

First, by way of empirical illustration in the world of our experience, we take all conditions to have been found fulfilled which would *warrant us in recognizing a structure of* acorn-morphology (a), and a structure of egg-morphology (e) as potential producers of oak-structures (o) *and* bird-structures (b), respectively We know, too, that the biologist's taxonomy has so defined the morphological classes by a-structures and e-structures, as to make the two classes no

less exclusive one of the other than the geometer's classes, square-structures (s) and circle-structures (c). (As the formal logician would express himself, both the class, ae-structures, and the class, sc-structures are, by definition, null-classes). In this particular example, the morphologies of the potential products, o and b, are no less incompatible than those of their potential producers, a and e, but our general definition of a functional class, while it *requires* the incompatibility of the potential producers (u and x), merely *permits* the incompatibility of the potential products (v and y). Yet, with all the structural incompatibility of the products, a and b, they will be seen to have a non-structural property in common each product belongs to the same morphological class as the structure that produced its producer An oak-producer-acorn and an oak-product-acorn are of a common o-morphology, a bird-producer-egg and a bird-product-egg, of a common b-morphology both potential products are potential producers This nonstructural property shared by the potential products, confers a correlated non-structural property on their respective producers both a-potential producer-o, and e-potential producer-b, are *potential reproducers* Under these conditions we say, the two morphologically incompatible classes, a, e, are species of the common genus, *structures having the function of reproduction*, or, for brevity, the genus *functional reproducers* To this same genus belong all species recognized by the biologist to be morphologically incompatible classes of potential reproducers

Generalizing, Postulate IV lets the symbol (ρ) stand for any nonstructural condition fulfilled by the potential products (v, y) of any two morphologically incompatible classes (u, x) of potential producer, and recognized in classes, u, x, two species of the genus, *functional ρ-producers*.

Under the same functional genus as u and x, it subsumes as species any third class of potential ρ-producer, related to u and x as these are related to each other This definition of functional properties will, one thinks, be general enough to accommodate all such classes and properties as the bio-centrist may need for the description of his world of functional bodies

The meaning of the conditions imposed by IV on functional producers and the range of classes seeking these conditions in the world of our experience would now seem to be clear enough, but does not its very clarity reveal an implication inconsistent with the accepted uses of speech? It is the implication that in a system holding but a single one of the morphological classes to which we now attribute *both* a potentiality and a function of producing a ρ-conditioned product, Post IV allows us to attribute *only* a potentiality, not a function of production For example, we now attribute to structures of acorn-morphology and of egg-morphology, respectively, not only the potentiality of producing, the one an oak, the other a bird, both reproductions, but also the function of reproduction In the same functional genus as acorn and egg, belong all morphologically differentiated species of ovum-containers Suppose, however, experience had discovered to us but a single one of these species of functional reproducers, the acorn, say, and no other, would the acorn have lost, or rather not yet attained the function of reproduction, just because it was the only known class of potential producer *whose producers and products were of the same morphology? Common use would not insist that acorn structures would be devoid of the property they possess in common with egg- and a countless variety of other ovum-containing structures,* merely because no other class of structures was known to

share this function with the acorn Yet IV does insist on just that point Why?

It is because our study assumes any scientific taxonomy to exclude from its vocabulary synonymous terms If we knew but one morphological class, x, to be potential producers-ρ, all the needs of scientific classification would be met by the class-name, *x-potential producers-ρ* to call the same class, *x-functional reproducers-ρ*, would be to introduce into our nomenclature a sheer redundancy of terms Whereas, informed of the existence in our world of two morphologically incompatible classes, x, y, of such ρ-producers, neither of the class-names, x-potential ρ-producer, nor y-potential ρ-producer, would apply to a class including in a common genus both these species, together with any others of which experience might later inform us Yet, nothing is more important to the biocentric sciences than to provide themselves with just such generic names, independent, as each one is of any implication of structural conditions This need, the class-name, *functional reproducers*, exactly meets and meets without danger, indeed, without possibility of falling into a redundancy of terms

To return, now, to the question whose consideration was postponed Why interpolate the category of *potential production* between those of *possible* and *functional production*? To answer, consider the information concerning conditions of production and nonproduction accumulated by one who finds his world to hold objects falling within one after another of the three modal classes, *possible, potential, functional producers*

(1) To have found his world to hold among the x's, which have as yet produced no y, an x-possible producer-y, one must have found that world to include at least one x-actual

producer-y, and at least one x-actual nonproducer-y Since the two x's found to be actual in their production or non-production are of the same structural class, it follows that future production or nonproduction is *not determined by the structure of any x-possible producer-y.*

What, then, does determine this issue? To one no better provided with factual data than the subject of par. 1, a possibility remains open. Since the x-actual producer-y, and the x-actual nonproducer-y are located in the same natural system, and since no two locations in any one system can have identical environments, may it not be that the issue between the future productivity or nonproductivity of a possible producer is determined by its environment? The conception of a point-property determined solely by the point's environment, would be nothing new to a science that understood all modal properties of production to be attributes of physical bodies The physics which all bio-centric sciences presuppose, would already have attributed to points, material or spatial, allocated in a field of force (gravitational, electrostatic, electromagnetic, etc), the pro-perty of have a *potential* Nothing, then, in the biocentrist's previous experience of physics, would exclude the possibility that a possible producer's prospective productivity or non-productivity was determined by its environment

(2) This possibility no longer remains open to one who has found his world to hold, among its x-possible producers-y, an x-potential producer-y For to be an x potential producer, an x-possible producer must be located in a world in which it coexists with at least two x-actual producers-y Since any two x-producers-y, no less than an x-producer-and an x-nonproducer-y, must exist in different environ-ments, it is no more possible that an x should be determined to actual y-production or y-nonproduction by its environment,

than by its structure What, then, does determine the issue?

Of course, we know the answer Whether the total cause-distribution in which an x is a detail, shall or shall not be followed by an effect-distribution in which a y is detail, under conditions that place the x and y in a producer-product relation one to the other, is determined by the laws of the natural system in which these two distributions are moments The outcome is determined neither by the structure of the x-detail, nor by that of the x-environing detail (x̄) found in the cause-distribution, but by the sum of the two (x + x̄), i e by the total cause-distribution But this conclusion, all our previous study has prepared us to accept as an implication of the reciprocal determination of cause and effect Upon which, one may be moved to ask What can it advantage a science, the test of whose adequacy lies in its ability to predict future events, to introduce this new producer-product relation, seeing that neither knowledge of a given structure (x), nor knowledge of its environing structure (x̄), taken singly, could serve as datum determining whether or not the future history of the system to which they both belong would witness a subsequent moment at which was to be found a y-structure fulfilling the conditions laid upon a y-product-x? This future event could be predicted only on the basis of such knowledge *of a structure and its environment* (x_1 + $\bar{x}_1$) *as would sum up to a complete knowledge of a certain cause-distribution* Then and only then could we tell whether or not a subsequent effect-distribution (y_1 + $\bar{y}_1$) would include a y recognizable as a y-product-x All of which throws the possibility of predicting future events back on the shoulders of the cause-effect relation, where it has lain from the beginning of our discussion Of what use, then, the

producer-product relation, or any of the modal categories that depend on it for their meaning? The question is only to be fully answered after the introduction into the vocabulary of our discussion the next and last of our modal categories, that of *probable production.*

(c) *Probable production.* Post. v, conditioning this category, presents no difficulty to the understanding of the meaning, it does, however, raise a baffling question as to its proper application. The postulate makes it plain enough that one who has found in his world a possible number n of members of the x-class, of which a number k greater than zero and less than n have, and n-k *have not* produced a y, would in general estimate the probability of any $(n + 1)$th x-producing a y to be k/n But it would be far from supposing that as the number (n) of x's, found to be actual producers or actual nonproducers-y increased indefinitely, the value of the vulgar fraction, k/n would remain constant. How, then, make explicit the sense in which he intends to let *some* quantity, say, q, calculated after the observation of but a given number of x's, establish, for the unknown number of x-possible producers y included in a given natural system, the quantification of the category, x-probable producers-y, with a probability of production q?

To find a defensible answer to this question, is a problem that pre occupies all the statistical scientists of our day It is of importance to an understanding of experimental method, not excluding the method to be employed by the metric sciences The proper solution of the problem is not an issue that need detain us here, except to note that such a solution fulfils the promise made in Part II, the promise, namely, that the question as to whether the definition of *truth* and *reality* there accepted as the meaning implied by the metric scientist's use of the term and practice in effecting

progressive approximation, would hold for the statistical sciences as well By-passing the discussion of the entire question How to determine experimentally the value of q that should quantify, for any given natural system, the class-name, *x-probable producers-y, with a probability of production q?*, let us content ourselves for the moment with adding a note of comment whose truth is independent of the outcome of that inquiry

(d) This note does no more than call attention to the significance to scientific method of a fact already remarked the fact, namely, that the modal categories, *possible, potential, functional production*, are, all of them, unquantified If this were not so, their service to the taxonomist of the biocentric sciences would be lost In contrast to the ever-changing value the statistician finds for the probability of production, the inclusion of a given x-structure in any of the classes, possible, potential, functional producer-y, once established, remains unchanged through all variations of statistical returns Obviously, if, in a given natural system, but one x-actual producer, and one x-actual nonproducer-y has been found to coexist with an x not yet recognizable as either the one or the other, it makes no difference to the permanent inclusion of that x in the class, x-possible producers-y, whether later statistics should have found a billion producers to the single nonproducer, or a single producer to a billion nonproducers and what holds for the genus holds for the species, classification of a structure under the head of potential, or of functional producer, remains unchanged through whatever changes *statistics* may bring to the value to be ascribed to any probability of production [3]

(e) An illuminating, if not indispensable comment would consider the measure to which our five definitions of modal categories conform with a requirement a careful logician

would lay upon any admissible system of definitions He would insist, namely, that if two definitions provide different connotations, then the denotations may differ as well. Thus, a class of structures whose connotation is completely given in structural terms must be guarded against the possibility of having a denotation identical with that of a class whose connotation cannot be completely given without the use of nonstructural terms. This condition, is, of course, imposed upon all systems of definitions, upon those offered in Part II by mechanical, physical, morphologically defined classes, no less than on the classes of modal producers with which we are here concerned Not to interrupt the discussion of the definitions just offered of these new terms, a re-examination of old definitions with this new test in mind may be left to the care of a critical reader

Reviewing the newly offered set of definitions covering the five categories of modal production, we may begin by asking whether in any natural system the membership of an x-class, defined in purely structural (morphological) terms, could prove to be composed exclusively of x-possible producer-y, a class whose definition cannot be completed without composing its connotation of the structural property, x, and the nonstructural property, possibility of y-production Evidently, in no natural system could such an identity of the denotations of the x-class and of the x-possible producers-y class (as conditioned in Postulate II) be observed, for before there could be found a single x-possible producer-y, there must have been found at least one x-actual producer-y, and at least one x-actual nonproducer-y. Consequently, any system containing a class of x-possible producers-y, must also contain one or more of x-actual producers-y, and the denotations of the two terms are mutually exclusive This, of course, does not exclude the

possibility that at any given moment in the history of a natural system, the x's belonging to that system shall become either actual producers- or actual nonproducers-y. Such, indeed, is the present status of all historic ovum-containers, once the possible-reproducers of a biological species now extinct On the other hand, any given moment of a system's history may reveal no members of an x-class, save x-possible producers-y. Such would be the case of all existing x-possible producers-y, had all x's that had been actual producers-, or nonproducers-y perished before this day.

What is here shown true of the whole class of x-possible producer-y must, of course, be true of subclasses of this class, the x-potential, probable, functional producers, none of these species of possible producer could include members of the x-class not included in the denotation of the genus of possible producer under which each of them is subsumed

It may be thought, however, that the logician's caution to assume non-identical denotations to terms of nonidentical connotation has not been observed in the modal categories, possible, potential, and functional production Take, for example, a system in which the existence of a class x-potential producers-y has been established In any system, all x-potential producers-y remain x-possible producers-y, and in this system, all x-possible producers-y, are x-potential producers-y Yet the connotations of the two classes, x-possible- and x-potential producers-y, are not identical Have we, then, excluded the possibility that these two classes of nonidentical connotation might have identical denotation?

But the answer to this pertinent question lies close at hand The genus, *x-possible producers-y* of which potential and nonpotential producers are mutually exclusive species, is not the genus composed of all x's of given morphology

that in a given system are found to be possible producers-y,
the two species in question are not two mutually exclusive
sub-classes of that x-class The genus, here, is the generic
Class of classes (C), formed by giving to x and y all possible
morphologies attributable to structures that, in a given
system, stand in the relation x-possible producer-y The
logician's requirement is met, if of the classes falling within
this generic Class (C), some belong to the one, some to the
other of the mutually exclusive species (C_1) x-possible *and*
potential producer-y, and (C_2) u-possible *but not* potential
producers-v. To the first species, C_1, would belong all types
of x- and y-morphology of which a given system had been
found to contain *at least* two x-producers-y, and at least one
x-nonproducer-y To the second species, C_2, all types of u
and v which in a given system had been found to contain
but one u-producer-v and at least one u-nonproducer-v So
defined, it is impossible that the Classes, C_1, C_2, should have
so much as one class in common, let alone identical denota-
tions Nor is it difficult to convince oneself that, in the world
of our experience, neither of the two Classes is empty, in
the space-time environment of a class falling within either
Class, we find a class falling within the other Examples will
so readily occur to one, that their finding may safely be left
to any who care to seek them

The present chapter has developed a technique which
should, one thinks, disembarrass our thought of the first of
the historic difficulties in the way of adjusting our accepted
notion of the biocentric world to the requirements of
mechanical imagery This difficulty arises out of a confusion
of the kind of "inherence" of functional properties, with
the "invariance" of structural properties By way of testing
the contribution made to the resolution of this first historic
difficulty it will be well, before turning to the second, to cast

a backward eye over such progress as has been made toward effecting an adjustment of the world of functional objects to the demands of mechanical imagery.

At least this much seems to have been accomplished. Functional classes as they now stand defined, can not only exist in a mechanical medium, but can exist only in a mechanical medium. Briefly to recall the grounds of this statement; (1) a functional class can exist only where structurally incompatible classes of potential producers exist; (2) a class of potential producers can exist, only where at least one actual nonproducer and more than one actual producer exist; (3) an actual producer can exist, only where a producer-product relation can be established between some structure x, and another structure y; (4) a producer-product relation can be established between a given x and a given y, only where the x is recognized to be a detail in some momentary cause-distribution, the y, a detail in some momentary effect-distribution of a (closed) natural system; (5) but a cause-effect relation can by definition exist only between two momentary distributions of a mechanical image of a natural system, and, by implication, between two physical or other images of such a system as have been found adjustable to at least one mechanical image thereof. Hence our conclusion; objects falling within a class defined in terms of function, can exist in and only in a world of which all empirical observations can be adjusted to a mechanical image of some closed system constituting part or all of that world.

Having set down for future reference this brief note on such progress as the present chapter may have made towards the resolution of the first of two historic difficulties, the next chapter may attack the second. It was not without labor that the first of these obstacles to scientific progress was overcome,

it can hardly be less costly of effort to remove the second
How, indeed, are we to adjust to the demands of a deter-
minate mechanism, our conception of a biocentric world, to
some at least of whose constituents we are unwilling to deny
a certain "indetermination" or "spontaneity" of behavior?
That is the problem now facing our study

¹The symbolism throughout preserves the convention originally set
(Chapter 17) by which arabic numerals indicate space coordinates,
letters time coordinates, individuating members of the morphological
class represented by a literal symbol to which they are attached as sub
scripts

²Probability of production can indeed be possessed in various degrees
but knowledge of a structure's probability of production is not pre
supposed by the recognition of any functional property we attribute to
it For the rest, wherever degrees of probability are in question they
are recognized as varying with variation in the environment

³To have established these, or analogous permanent categories of
classification, is the great service Aristotelian thought rendered to bio
centric sciences There is, therefore, historic injustice done Aristotle and
the whole scholastic tradition based upon his methods by the contempt
in which Post Baconian and more pronouncedly, Post Galilean com
ment held all philosophies that, preceding Galileo, contributed little or
nothing to the development of experimental metrics How little this
limitation reflects on the intelligence of Aristotle and his followers, is
realized so soon as we remember how long the world had to wait after
the mathematics of measurement were fairly well in hand, before the
mathematics of probability were made available

20. Functional Classes: Indeterminism

THE PRESENT WRITER IS ACCUSTOMED TO FIND THE EYES of a serious company turned questioningly on him, whenever, by way of introducing the difficult topic now before us, he has proposed to set the company "a little conundrum." To men well acquainted with the place this topic has in historic and current debate, the proposal could not but seem frivolous and in doubtful taste. How far from frivolous the intention of the proposer, will appear in the end; whether or not tasteful, his way of presenting the matter at issue in the form of a concrete illustration has at least the merit of opening a difficult debate with a clear picture of the issue to be debated.

The "conundrum" is this: The proposer is thinking of an individual object, c_1, belonging to a class of objects, c, which class itself belongs to a Class of classes C. What is the highest class, c, to which c_1 could belong; what the highest class, C, to which c could belong, if c_1 and c fulfilled the following conditions:

1. The c-class is susceptible of no structural definition.

2. The c-class is subject to laws susceptible of no physical explanation.

3. Under these laws, the past of a given c_1 may be explained; its future cannot be predicted.

4. The most probable future of any c_1 is a function of its past.

There are other characteristics of the c_1 and c that the proposer has in mind; but those already listed have generally proven sufficiently reminiscent of certain historic reason-

ings concerning a class of objects generally considered to fulfil the four conditions stated, to lead one or another of any well-informed company to offer a ready *solution of the riddle* It is a solution that has behind it the greatest weight of historic approval, for do we not recognize in the fulfilment of these conditions the characteristics on which the "vitalist" (or his equivalent in the nomenclature of whatever historic period) chiefly *depends for evidence of a condition shared by all living things and by no inanimate things?* In which case, the highest class to which our c_1 can be assigned, is the class of living organisms

And yet, the individual object of which the proposer of the riddle professed himself to be thinking, was *no living thing*, or organ thereof, or grouping of living things it was just the watch, ticking in his pocket Let us see whether that particular c_1 belongs to a class c, meeting the conditions laid upon c_1 and c in the formulation of our riddle —

(1) If asked of a sudden—*what is a watch?* would not our immediate answer be—A *species of timepiece,* i e one species of a genus of objects whose function it is to tell time? And is this functional class, *timepiece,* susceptible of structural definition? What *structural description* fits as genus whose familiar species include the sundial, hourglass, clepsydra, pendulum clock, spring watch, electric oscillator—to mention only the species now in common use, without putting any limit on the varieties of timepiece *the future may invent?* No structure, surely, is common to all these "*instruments of time,*" yet shared by no other class of objects falling within our experience

If this insusceptibility of structural definition *were not true of the class timepiece,* the class so called could not qualify as a functional class of any kind For, recalling the condition imposed (Post IV) on such a class, every

functional class is required to be a genus composed of more
than one species of potential producer, differentiated by their
incompatible morphologies All that is common to the species
of any functional genus is the requirement that their poten-
tial products fulfil a certain nonstructural condition (ρ) What
ρ-condition is to be met in all species of timepiece, is no
more difficult to specify for the functional class timepiece
than it is for the functional class *ovum-container*, only, use
and custom have left the condition imposed on the potential
products of all timepieces less ready to hand than the
requirement of being reproductions, imposed upon the
potential products of all *ovum-containers* Consider, how-
ever, "my watch" (the c_1 of our conundrum) the minute-
hand is now at a measured angle with a diameter passing
through the 12- and 6-marks on the dial, a little while, and
the minute-hand will make another measured angle with
this diameter, from the difference between these two angles
we shall be able to judge that a period of five minutes has
elapsed between the *then* and the *now* From which, and
from our experience of other watches that have "kept
going" for the space of five minutes, we may describe any
present watch as a potential producer of a future self, so
differing in attribute from its present self that the difference
between present and future attribute is a known function of
time This definition we may take to cover the class of all
possible timepieces In most timepieces in current use, the
attribute whose change between any given "now" and and
given "then" measures the time-interval between the
two moments, is an angle-reading, in some (the hour glass
and simpler types of clepsydra) the significantly changing
attribute is vertical level, on those the future may develop,
our definition places no restriction as to the attribute whose
change may be used to measure a lapse of time

We see then, what is the ρ condition to meet which is the necessary and sufficient requirement imposed of an unrestricted range of varistructural potential producers of varistructural products constituting the genus, *structures whose function is to keep time* Or rather, the category, functional producer, while it places no upper limit, does impose a lower limit on the number of structurally incompatible species of potential producer that may form a genus of functional producers their number can not be less than two It is this lower limit placed on the number of species that may compose a functional genus, which establishes the impossibility (stipulated in Condition (1), supra) of offering a structural definition of a functional class

(2) Turning now to the second condition imposed upon members of this functional class, c = timepiece, its wording is not so unequivocal as to convey its exact meaning What precisely does it mean to say that members of the c class are "subject to laws susceptible of no physical explanation?" Do the laws, here too hastily and briefly characterized as *physical*, include any and every "law" the physicist may formulate on the evidence of his experimental observation? Does it, for example, include such statistically founded generalizations as the "second law of thermo dynamics" and whatever generalizations may be accepted by other departments of physics as are similarly based on statistical treatment of data? Our 2nd condition does not explicitly exclude such statistically founded principles as these, yet the thought of our puzzle proposer meant to exclude them, and to include under the head of "physical explanations" only such formulas as subsumed the behavior to be "explained" under a non statistical generalization The critical difference between the non statistical "laws" of physics, and the statistical "generalizations" which, though consistent

with, are not just an application of a non-statistical law to some special class of physical bodies, is easily stated. A non-statistical law prescribes the necessary future of *every* body of a class to which it is taken to apply. So, for example, does Newton's Law, require *"every* body in the universe to attract every other body, etc." (One recalls the moment when the behavior of Uranus having been found inconsistent with the requirements of Newton's Law, the physicist was left with no alternative but either, to discard this law as a law of physics, or, to find such hitherto un-observed facts as would, when taken as data, bring an enlarged solar system into conformity with the Newtonian formula. And, again, the discovery of Neptune having provided the required new fact, one will recall that later moment when the aberration of Mercury presented the physicist with the same alternative; this time not be to resolved by the discovery of new reconciling data, but only by that transformation of the whole system of individuating coordinates which has constituted one of the most recent developments of physical science. In neither of these difficult moments would it have occurred to any scientist to rest content with accepting Newton's or any other laws as furnishing a principle of sufficiently normal behavior on the part of all natural bodies to serve as the ultimate foundation of physics.

Compare with this the sense in which we accept any one of the many maxims popular experience offers for the guidance of practical people in their dealing with bodies expected to function. As any of them may be expressed in either of two ways. For example, put in one way, a prospective purchaser is warned that "cheap timepieces are poor ones." If this empirical generalization were understood to predict the early failure to function of *every* cheap time-

piece, there would be no market for such low-priced products, no one would spend a penny on an instrument that was *bound* to fail him at an unprofitably early date The fact that there remains a market for the cheaper grades of timepiece, shows how little the buying public expects *every* such timepiece to fail him "in no time" If the maxim has any general acceptance at all, it can only be taken to hold "for the most part" So it is with all statistically founded generalizations, to whatever class of bodies they may apply, and with whatever method they may have established

But the *same warning against* "cheap timepieces" may be worded in another way without change of meaning namely, "a cheap timepiece is *likely* to be a poor one" So worded, a statistical law, no less than a non-statistical law, holds for every member of a class to which it applies, but, unlike the non-statistical, a statistical law forecasts the probable, not the necessary future of any individual falling within the range of its application

It is to be understood then, that the second condition laid upon our c class, *is an abbreviated way of saying that* members of this class are subject to no law subsumable under a nonstatistical law of physics So far as subject to statistical laws, the class *timepiece* conforms rigorously to the condition that its members are subject to *laws susceptible of no physical explanation*

(3) But if the class *timepiece* conforms to the first two, does it also meet the third of the conditions imposed upon our c-class? Is it true that (a) by the laws to which timepieces are subject the past of an individual timepiece may be explained, but (b) its future cannot be predicted?

(a) It will be noticed that the assertion does not claim the past of *any and every* member of the c class to be thus explicable, but only that the past of *an* (= some) individual

c *may be* explicable. To return to the vernacular; if a watch purchased at some ridiculously low price "give out in no time", no one is surprised. One's only comment would be, "Naturally", or "What would you have?" meaning always that the watch's performance is *quite normal* to the class of such subpriced timekeepers. Which comment merely brings to our notice a matter of common acceptance; namely, that more often than not, we "explain" in terms of the *normal*; only in the formal "postulates" of science do we demand explanation in terms of the *necessary*. Only an individual's behavior that can at a given stage of statistical development be brought under no *known* norm established for any class to which the individual has so far been assigned, is taken to be quite inexplicable, and, as we are likely to think, "mysterious." Only when, for example, we consider the enormously precocious and prolific musical output of (say) a Mozart, does the phenomenon baffle our conception of what is normal to any as yet imaginable class of human being; only then are we mystified, merely giving a name to our bewilderment by calling Mozart a "genius." There have, one recalls, been periods in the history of philosophy (notably the "Romantic" period of the early Nineteenth Century German "systems") which insist on the ultimate impossibility of classifying a "genius" with any manifold of human beings to which his performance would be normal. But it would offend the caution of a statistical scientist to assume any kind of phenomenon that is now an "abnormality" to be beyond the reach of any possible reclassification of individuals to bring under the head of what is normal to *some*, as yet undefined class. Such, at least, is the attitude the present study has taken in the past, and will (unless otherwise advised by experimental statistics) preserve as the basis of its future reflections of the "mysteri-

ous, the inexplicable genius" of art, science, prophecy, or whatnot other baffling phenomenon of human, or other, nature In a word, the experimentalist accepts as a thesis, consistent with many a past experience of scientific progress, the general principle that no evidence can justify the assumption that any abnormality which now "mystifies" the statistician, must remain a "mystery" to all future statisticians

It is, then, in a well-recognized sense that the past history of an individual timepiece *may be*, not *must* be, explicable by the statistical laws which apply to this, as to every other functional class Only such individual past histories are explicable as experience shows to be *normal* to some class to which the individual has been assigned Such as are abnormal to every old class recognized by a given taxonomy, the statistician sedulously seeks to bring under the head of what is normal to some new class, not yet recognized by our taxonomic methods

(4) The fourth and last condition is easily seen to be fulfilled by the functional class *timepiece*, and equally by every functional class that may be found to fall within a biocentric world It is to the effect that "the most probable future of any c is a function of its past history " Who, indeed, would bet on the future performance of a given watch, fiddle, automobile, or other instrument of the arts serving the purpose of man, in ignorance of its age, previous use, reputation of manufacturer, etc? This knowledge of the past history of such an instrument is not said to be indispensable to *any* calculation of the instrument's probable future, it is said to be essential to *an estimate of the instrument's most probable future* All, one thinks, will allow that our daily practice of "counting on" or "not counting on" the future functioning of an instrument we are about to use recognizes

the importance to our expectations of this score of such
knowledge of its past as is here in question.

What historic difficulties and dangers are connected with
this known dependence of the most probable future on the
actual past history of a functional individual, spring from
another source. It is easy to give the principle just analyzed
another wording; easy, namely, to put it, that the most
probable future "conduct" or "behavior" of an individual
depends upon its "past experience." And there is nothing
wrong with this way of expressing oneself, so long as by
"conduct" or "behavior" one means one or another kind of
functional performance; and by "past experience" one
means no more than "what has befallen" the individual in
question. There is, however, a danger associated with this
locution, a danger that future discussion will show to have
introduced confusion and mystification in many an historical
discussion of the factors influencing living, and, above all,
human behavior. It is the danger of limiting "past experi-
ence" to such past history as may be in some sense
"remembered." What, indeed, could one learn from an
utterly forgotten past, that might serve as a guide to a pro-
spective future? Hence, the appearance in history of such
mystifying adjectives attached to "memory" as "uncon-
scious" memory; or such locutions as, anxious to avoid the
"unconscious," take refuge in a philosophy of the *als ob*—a
philosophy content to note that everything to be accounted
for in terms of the past of an individual, takes place *as
though* guided by a memory, even though there be no
memory, conscious or unconscious, that could be supposed to
guide it. It should help us when in future contexts we shall
have· to consider historically imported invocations of, or
substitutions for "unconscious memories," to recall that
dependence of the most probable individual futures on

individual past histories is of no less importance in prognosticating the future of inanimate than the future of animate and, in particular, human constituents of the biocentric world. Here, the only "memory" that can come into question is the memory of the prognosticating mind, not that of the object whose future is prognosticated.

One sees, then, that such bodies as the physicist has found adjustable to mechanical imagery, so far as their physical attributes and controlling physical laws are concerned, do not cease to be adjustable, *by reason of their possessing functional attributes and consequent subjection to "nondetermining" laws of a statistical order* as well as to "determining" laws of physical order. But these determining laws could not be physical if they were not adjustable *to the ultimately determining laws* which establish a cause-effect relation in, and only in, a mechanical image to which all observation of any intelligible natural system must conform. And it is to be supposed that no one will take the functioning of a watch, or the inavailability of any but statistical laws for the prognostication of *its probable future,* or the dependence of its most probable future on the history of its past, to imply that a watch, in order to function, and be subject to non-determining laws, must violate the requirement of being adjustable to some pattern of mechanical imagery. *Instruments of the arts are generally supposed to function just because their structure and functioning do accord with the universal mechanism of Nature.* If then, the living components of the biocentric world can "live" only by violating this universal mechanism, it can only be because *the definition of life connates other properties and implies other kinds of indetermination* than such as are common to all functional classes, animate or inanimate. Whether the living do possess such properties and do exhibit

such indeterminism as would be consistent with the exist-
ence of living bodies in a mechanical medium, is only to be
judged after our acceptance of some definition of the class
living being, and some conclusion as to what the "spon-
taneity" of living behavior implies That the spontaneous
movements of the simplest organism do mean some-
thing more to us than the "inexplicable," because abnormal,
vagaries of a watch, is evidenced in our habitual manner of
speech Such spontaneity as we recognize in the gestures of
an amoeba, we would not seriously attribute to a sudden
abnormality of performance on the part of a watch, though
we readily fall into a metonomic use of terms implying spon-
taneity in speaking of the "vagaries," the "waywardness" or
(with humorous impatience) the "cussedness" of any type of
inanimate instrument expected to, but unaccountably failing
to, perform a specific function What more than a kind of
indetermination common to all functional beings is implied
by the *spontaneity* of living behavior in general, by the
freedom of human will, in particular, remains to be seen
an understanding of *animate spontaneity* presupposes a
definition of life, an understanding of *human freedom*
presupposes a definition of *man* All that is for the future,
for the moment it is enough that the proposer of our "con-
undrum" is now in a position to sum up its solution

The c_1, which the puzzle setter happened to have in mind,
was a watch, it could have been a member of any functional
class, i e any class whose definition required the attribution
to its members of some functional property It could then
have been a member of any one of those classes whose
denotation includes all constituents of the biocentric world
One Class of classes to which the c-class of our problem-
setting must belong, is, we see, the Class of all possible
functional classes But our question was not, What is *a*

Class?—it was, what is *the highest* Class of classes under which our c-class could be subsumed? And this, the class of all functional classes is not The two contradictory Classes under one or the other of which every class of natural objects must fall, were indicated at the outset of Part III (Chapter 17, p 261) to be the *structural* and the *non-structural* Classes, i e the Class of classes whose connotation can be given in terms of exclusively structural properties, and the Class of classes whose connotation implies their possession of at least some nonstructural property To the former Class belong, in classic systems, all classes of bodies having the same mass, volume, velocity, to the latter, all classes of bodies acted on by the same force (i e bodies the product of whose structural mass and nonstructural acceleration is identical), or bodies having the same acceleration, or (as we now see) bodies having the same function The last question to which our puzzle solver must find an answer is now answered, the most inclusive Class of classes to which a c class meeting Conditions 1–4 can belong, is the Class of all possible nonstructural classes

With this, the solution of our riddle is complete, but of course the riddle was set for no other purpose than to introduce in a form more likely than another to uncover some details frequently *overlooked* by historic reflection on the general topic of the constitution of a biocentric world and its adjustability to the requirements of mechanical imagery For example, one's *first reaction* to our conundrum is almost sure to overlook the fact that Conditions 1–4 may be as rigorously met by an inanimate thing as by a living body And this is for a *good reason* it is so easy to pass from an accurate to an inaccurate wording of these conditions without *detecting any difference* in the meaning of the two wordings Would one be doing more than translate a pedantic

formality of phrasing into the idiom of easy discourse, if one were to restate the conditions laid upon the class c to which our individual c_1 belongs in a form requiring (1) the class of things in question to be *indefinable*; (2) the behavior of its members *inexplicable* and (3) *unpredictable* before-hand, yet somehow *understandable* after the event, particularly if (4) we *know enough of the past experience* of the individual so behaving? So worded, the conditions are strictly reminiscent of those the vitalist commonly supposes to be met by the class *living being*, and only by the class *living being*. And, no doubt, a being of this kind would be susceptible of representation in no sort of mechanical image; it would be "mysterious" from any point of view. Naturally enough, one long accustomed to accept as ultimate, and perhaps not unwelcome "the mystery of life", would look with keen suspicion on the reasoning here offered to show that the accurately worded conditions laid upon our c-class were no more mysteriously met by all living beings than by all watches, clocks, fiddles, saws, and waffle-irons. To the present study, the question of the mystery or non-mystery of life, as, indeed, of the whole biocentric world, reduces to the question of the adjustability or nonadjustability of this world to the demands of mechanical imagery. So far, our reflections have uncovered no insuperable obstacle to such adjustment. But let our last word on this issue await the last word of the present chapter; before coming to the last word, one more note of interest to the sequel.

It has been shown that every functional class is subject to statistical laws, by which the probable future of an individual may in all cases be predicted, its past in some cases explained. To have shown this much is not to have asserted or implied that such statistical laws are the *only* laws to which a functional class is subject. Yet, with one obvious

reservation, this assertion would be true With the reservation made explicit, the principle to be accepted takes the form The only laws that hold for a functional class, and *do not hold* for any structural class under which that functional class may be subsumed, are statistical in nature. To have so defined a nonstructural class as to make a structural definition of that class impossible, is not so to have defined it as to deny to its members the common possession of any one structural property. That, of course, would have been an absurdity All functional classes are composed of physical bodies, and all physical bodies have some structural properties in common In a Newtonian system, wherein to *every* body is attributed the property of gravitational mass, every member of any class of bodies, functional or nonfunctional, must share in the property of mass Whatever laws, then, hold for all massive bodies, hold for timepieces, fiddles, amoebas, men With this understanding of its meaning, our principle may not be more compactly worded The only laws that hold *exclusively* for functional classes are statistical in nature

Recognizing this principle, *one sees that the future of any body possessing functional properties is characterized by a kind of "indetermination,"* now carefully analyzed and explained But does the kind of "inherence" enjoyed by functional properties, or the kind of "indetermination" to which the prospective behavior of a physical body possessed of functional properties is not subject, preclude the possibility of adjusting such a body to the requirements of mechanical imagery? *The argument of the last two chapters leads to no such conclusion On the contrary,* neither the kind of "inherence" accorded *functional properties in the last chapter,* nor the kind of "indeterminism" recognized in this chapter as characterizing the behavior of functional

beings, bars the adjustment of our experience of functioning bodies to the demands of our formal pattern of such bodies. But, clearly enough, any functioning bodies that do not conform to the functioning bodies of our pattern can not only exist in a mechanical system, but can exist nowhere but in such a system. The reasons for this assertion were fully set forth at the close of the last chapter, but, because of their importance to the sequel, may well be repeated here in a set of propositions that fall into the form of a sorites:

(a) Every functional class is a genus composed of more than one species of potential producer, differentiated by their incompatible morphologies.

(b) Every class of potential producer is a class of possible producer.

(c) Every possible producer of a given morphology coexists in a natural system with at least two bodies of the same morphology as itself, of which one is an actual producer, the other an actual nonproducer of bodies like one another in structure.

(d) Every actual producer of a given product is a detail in a cause-distribution, every product, a detail in an effect-distribution of a (closed) natural system.

(e) Every natural system in which a cause-effect relation exists between two of its momentary distributions must be adjustable to at least one mechanical image, for only in a natural system adjusted to a formal mechanical schema can a cause-effect relation exist.

(f) So, only in a natural system adjustable to a mechanical image can a class of bodies possessing functional properties exist.

This result sums up all that the definitions and arguments of the last three chapters sought to establish. With it are resolved those historic difficulties which have led many to

doubt, and some to deny, the possibility of adjusting physic
bodies of a class whose connotation implied the possession
functional attributes to a mechanical image of whatev
pattern and point-distribution But as the denotation
classes having a functional connotation exhausts the man
fold of physical objects making up the biocentric world,
follows that the existence of physical bodies constitutin
such a world is possible if and only if all the data gathered b
our observation of it are adjustable to a mechanical image o
the natural system of which it is a part

Arrived at this point, several possibilities open themselve
to our imagination, each of which invites its exploitation a
a suitable next step in study of experimental method These
possibilities present themselves as problems to be set and
discussed in categories new to these pages, but old to the
pages of history For example, the probable futures which
the statistical laws applicable to functional classes allow us to
calculate, are principally futures implying a *better* or *worse*
functioning of the functional beings they concern In other
words, we are on the threshold of the world of values Why
not penetrate as deeply as we can into that domain which
has had such deep interest for all historic reflection? Or
again, one may suspect that it would take but a step to pass
from the concept of a class whose connotation implies that
each of its members is expected to perform a function, to the
concept of a class whose generic connotation implies an
expectation that *its members achieve an end, the species of*
this genus being differentiated by *the diversity of the means*
they severally employ to achieve this end Are we not then,
invited to look deeper into that historically most interesting,
but most obscure topic of a *teleological* interpretation of
natural phenomena?

These and other problems of world old reflection do

indeed invite us to step over thresholds at which we feel
ourselves to have arrived, and penetrate at once into dark
interiors But, so doing, would be to leave only half, or less
than half, performed the task which has been for some time
engaging us—a task, which is, indeed, but a late chapter in
an investigation that has preoccupied us from the beginning
The general question of the dependence of experimental
method on our ability to adjust observational data to one or
another mechanical image of a natural system, brought our
reflections on history into contact with one after another
difficulty that historic science has met and overcome in
attempting to effect such an adjustment The last of these
difficulties encountered and disposed of, presented itself in
the form of a certain indeterminism recognized in the
behavior of all classes of functional objects making up the
constitution of the biocentric world But inasmuch as such
indeterminism as affected all classes of functional beings is
no more than the consequence of a simple condition holding
for all such classes, it implies no impossibility of giving to
each member of a functional class its place in a determinate
physical world order It is true enough that the only laws
holding exclusively for a functional class are laws of pro-
bability That does not make it the less true that each
structure belonging to that same class is subject to physical
laws and adjustable to the demands of mechanical imagery
But in the "spontaneity" of life in general, the "freedom"
of man in particular, custom has recognized some deeper
sort of indetermination than such "waywardness" as we
readily associate with the behavior of every inanimate
instrument of the arts All of which seems to commit us as
the next step in a study of experimental method to an
examination of that indeterminateness of living behavior
which history has rightly or wrongly distinguished from the

unpredictability of inanimate functioning. But to be committed to this inquiry is to be bound to a search for the definition of a concept which all history shows to have been one of the most obstinate in baffling the analytical powers of man: the concept of life. To a first step in the direction of this definition, the following chapter devotes itself.

21. Attributes of Living Bodies: Historical

THE PRINCIPAL CONTRIBUTION OF CHAPTER 19 WAS THE construction of a formal pattern that should meet two conditions (1) that all bodies of our empirical world possessed of functional properties should be found adjustable to this pattern, (2) that the pattern itself should conform to all requirements imposed on a mechanical image of a natural system To be possessed of functional properties is the condition that differentiates the constituents of a biocentric world from such other constituents of a natural system as are devoid of all but physical attributes, what remains to be constructed is a pattern to which can be adjusted those constituents of a biocentric world that may aptly be said to compose its *biotic center* Within this center would be included only such constituents of a biocentric world as were possessed of *life*, from it would be excluded such other constituents of that world as, though themselves lifeless, could exist only as coexistent with living beings, e g the instruments, organs, social groupings of life

Since the living constituents of a biotic world must exist as details of that biocentric system of which the biotic is to be the "center," and as it has already been shown that a schema to which all functional bodies are adjustable is itself susceptible of representation in a mechanical image of the natural system in which these bodies exist, it follows that any schema we can construct of a world of living things differentiated from the nonliving in terms of functional properties alone must conform to the requirements imposed

on all functional bodies So conforming, the part must, like the whole, be imaged in a pattern that can be designed and only be designed as part of a mechanical image.

The history of ideas leading up to this problem-setting has been told proposed solutions have also their history, of which the present chapter will recall so much and in such manner as is best fitted to the general plan and immediate purpose of our study We may, for example, propose a sort of intellectual experiment, with a view to spying on the land Could the formalist of Euclid's day (we ask ourselves) or of many a day thereafter have done for biology what Euclid did for geometry? Could he have designed a postulate-set that would accommodate all that was then known whether of mechanism or of life in such wise that life could dwell in mechanism and be of it without loosing the defining attributes that differentiated the living from the dead? Or we may ask ourselves this other question Suppose a scientist of today who should have learned all the lessons that the history of the centuries reviewed in Part I had gathered concerning the way in which by one or another device science had been able to adjust to one or another pattern of mechanical imagery the phenomena of the physical world, but suppose him to have been left in ignorance of anything the biology of the last hundred years had learned that might bear on a like adjustment of the phenomena of living world to mechanism—could such a hypothetical figure have succeeded any better than could a scientist of Democritus' and Aristotle's day in effecting a like adjustment of organism to mechanism? Let this hypothetical question suggest the plan of our "intellectual experiment " We would know whether the formalist who had accepted the proposals of Posts I–V could add to these such other postulates as enable him to adjust what was known

of life and its attributes a century ago to the requirements
of mechanical imagery, in such wise that the image should
represent organisms as possessed of just those attributes
that were common and peculiar to the class of living bodies.

To be enlightening, an experiment need not have been
successful; should the one that lies before us fail, its very
failure might make a promising contribution to our ultimate
progress. It might, for instance, suggest a way, otherwise
difficult to find, by which the scientist of today may be able
to succeed where the scientist of a hundred years ago must
have failed. With the thought in mind that such postulates
as the present chapter is to propose, may make that chapter
no more than an historical introduction to the discussion that
is to follow, we may look upon these postulates as tentative,
and to avoid possible confusion, we shall enclose these
postulates in brackets, with the understanding that if the
"intellectual experiment" for which they set up the appara-
tus succeed, the brackets go; if the experiment fail, the
postulates go. With this foreword of caution, we may pro-
ceed to formulate our postulate-set, continuing in the
practice of giving these proposals their final wording, leav-
ing whatever may be needed in the way of explanation to
later comment.

Postulates

[VI] Let there be an individual things,[1] R (*res*) that will
have passed through a sequence of structural states, x_1, y_1,
x_2, y_2, . . . x_n, y_n, such that
(1) each antecedent x will have been a producer of each
subsequent y, and each antecedent y, a producer of each
subsequent x; where
(2) the x-states and the y-states are of incompatible morpho-
logy, i.e., an xy-class = a null-class; and where

(3) no antecedent x_0 or y_0 stands in the same space time relation to x_1 and y_1 as do x_1 and y_1 to the subsequent states of the x, y sequence, i e, the history of R will have a beginning, then for $n > 3$, R will have an *autogenic function*, i e, the function of producing for itself a future which shall be a reproduction of its past

[VII] Let R be an individual body that will have passed through a sequence of states, x_1, , y_n, meeting the conditions of [VI], such that for some value of k, each subsequent state for the sequence, x_1, y_1, , x_n, y_n, will have greater mass than its antecedent state, i e for some initial period of its history, R will have grown heavier as it grew older, then for $n > 3$, R will have an autogenic function in the special form of an *ontogenic function*

[VIII] Let R be an individual sequence of bodies, of which each will have passed through an x state and a y state, such that

(1) the sequence of states, x_1, y_1, , x_n, y_n through which R will have passed, meets the demands of [VI] then

(a) for $n > 3$, R will have an autogenic function in the special form of a *progenic function*,

(b) of any two bodies of the R sequence, the antecedent will be *progenitor* of the subsequent, the *progeny* of the ante cedent

Comment

(a) Since producer product is a transitive relation, each antecedent x of the x y sequence is producer of every sub sequent y, each antecedent y, of every subsequent x There fore, with the exception of the first and the last x and y, every x is an x reproducer x, every y, a y reproducer y

(b) For $n > 2$, the structures x_n, y_n are possible reproducers of x and y respectively, since at least one x (x_2) and one y

(y_2) will be actual reproducers; one x (x_1) and one y (y_1), actual nonreproducers of x and y, respectively; there being no x_0 and no y_0 of which x_2 or y_2 could be reproductions [VI].

(c) For $n > 3$, the structures x_n, y_n will be potential reproducers of x and y respectively, since at least two x's (x_2, x_3) and two y's (y_2, y_3) will be actual reproducers; one x (x_1) and one y (y_1), actual nonreproducers of their respective kinds. [Post. III, Chapter 19.]

(d) Therefore, for $n > 3$, the structures x_n, y_n will be functional reproducers; since [Post. IV, Chapter 19] the x-y sequence will include two morphologically incompatible species of potential reproducer.

(e) By an extension of meaning, frequently resorted to by science to the end of word-economy, the property which by definition belongs to the last two products (x_n, y_n) of the $x_1 - y_n$ sequence ($n > 3$) is conferred upon the integral "thing," R, to whose history the x-y sequence belongs. With this, R ($x_1 - y_n$) acquires the function of autogenesis, i.e., as was explained, the function of producing for itself a future which would be a reproduction of its past. Such an extension of meaning is resorted to only as a counsel of convenience, and is permissible only when the sense in which a well-defined meaning has been extended is itself well-defined. It is to be kept in mind here and throughout the sequel that no *extension of meaning* pretends to be deducible from the *intension of meaning* accorded a term by formal definition. The sequel will, one thinks justify this use of privilege in view of the escape it affords us from a number of cumbersome wordings which would otherwise have burdened progress.

(f) Post [VII] imposes on a *single body* the conditions under which the body would exhibit an autogenic function in the special form of the ontogenic function. Post [VIII] imposes

on a *sequence of bodies* the conditions under which that
sequence would bear an autogenic function in the special
form of a progenic function. These two postulates, taken in
conjunction with Post, [VI], are, one thinks, sufficiently
explicit in their statement of the conditions which differen-
tiate the species *onto-* and *progenesis*, while subsuming both
under the genus *autogenesis*. They may, then, be allowed
to pass without further comment, while our thought turns
to a more critical topic The extent to which empirical
science can adjust its observations to the demands our
postulates impose upon bodies having the function of onto-
or progenesis. We ask Can empirical science "find" in
Nature bodies exhibiting either of the functions conditioned
in Posts. [VII] and [VIII]?

When we do turn to experience with this question in
mind, world-old history and world-wide use have two sug-
gestions to make Both a naive past and a scientific present
recognize two processes, commonly called "functions," as
normal, if not universal, manifestations of life *nutrition* and
reproduction To say that science subsumes the two pro-
cesses of nutrition and reproduction under the head of
"functions" is not to say that science will have come so to
call them only after having assured itself of the adjustability
of the phenomena it takes to be evidence of nutritive or
reproductive behavior to the formal pattern of any type of
functioning that our postulates have conditioned The need
of adjusting found material to made forms is rarely recog-
nized by the science of the past, save in its treatment of the
oldest, best-formulated, and most underlying sciences logic,
arithmetic, geometry, and to some extent mechanics The
test of the adjustability of empirically recognized processes
of nutrition and reproduction to one or another of the

formal patterns of functioning designed in our postulates is
for us to make

And it is not hard to convince oneself that the most readily
observed manifestations of nutrition and reproduction are
adjustable, respectively, to the formal requirements of
ontogenesis [VII] and progenesis [VIII] Thus, as meeting
the conditions imposed upon the $x_1 - y_n$ sequence of struc-
tural states prescribed in [VI] and [VII], we may point to
the contrasting physical conditions in which an organism
finds itself at moments of "depletion" (x) and "repletion"
(y) Posts [VI] and [VII] condition ontogenic functioning
on the recognition of "at least one" such sequence of
structural states in the history of an individual body This
condition is fulfilled by any one of the several sequences of
depletion and repletion which we find in the higher, and
more readily observable, forms of body we class among the
living moments following periods of fasting and feeding,
thirsting and drinking, exhaling and inhaling, waking and
sleeping If there is any form of life that exhibits no one of
these sequences of depletion and repletion, it is a form that
does not fall under common notice, but is reserved for the
close observation of the laboratory Of what the laboratories
may have found in the way of a life that exhibits no one of
the sequences of depleted and repleted states which would
meet the conditions imposed by [VI] and [VII] upon
ontogenic functioning, we shall speak later

No less readily does common experience observe in most
forms of life a phenomenon of "reproduction" that satisfies
the conditions imposed by [VI] and [VIII] upon progenic
functioning In the reproductive forms of organism, each of
a sequence of bodies begins its history with an incipient
structure x, and normally attains an adult structure y, such
that a succession of more than 3 of these bodies will con-

stitute a line of which the last term (and, by extension of meaning) the line itself will have the property of progenic functioning. To be sure, humanity's practical interest in living beings soon acquaints it with "hybrids" which, while they exhibit a nutritive function conforming to the pattern of ontogenesis, rarely if ever exhibit a reproductive function meeting the requirements of progenesis How the absence of progenic functioning from some bodies, which common use nevertheless includes in the class of the living, affects the problem of life-defining will be considered in due course For the moment, a matter of more immediate interest claims attention

The last two paragraphs pretend to offer no more than illustrations of two sequences of bodily states of which the first is exhibited in the nutritive, the second, in the reproductive processes of a wide range of living beings So far as these illustrations go, their testimony is consistent with the conclusion that the nutritive and reproductive processes of living beings conform to the formal requirements imposed by our postulates on ontogenic and progenic functioning, respectively To say that familiar observation favors this conclusion, is, of course, far from showing that the weight of scientific evidence confirms it As an adequate review of this evidence is far beyond the reach of a study of such general scope as the present essay, the wisest course open to us would seem to be to accept the conclusion which our own examination of this evidence supports, and to consider how far the acceptance of this premise helps us toward a definition of life Leaving, then, to a later context what more may be said in brief space on the subject of experimental evidence bearing on this premise, we turn to the question that naturally shapes itself at this point Will either or both of the two processes of nutrition and reproduction, conforming,

respectively, to the patterns of ontogenic and progenic functioning, suffice to define the concept of life?

The period that responded to the strongest motive for seeking an affirmative answer to this question lies well behind us It begins with Aristotle and lasts through all the centuries dominated by Aristotelian habits of thought If we were to reconstruct the ultimate motive that guided Aristotle's philosophy, that of explanation in terms of purpose, we should have no difficulty in recognizing a fundamental principle of his natural philosophy as a necessary first step toward the realization of his purpose That principle required him to define *all* classes of natural objects in terms of function, what we should now call functional terms, alone Aristotle, in terms of a distinctive "place" to which it was proper that each should return if displaced and left unimpeded, could follow no other course, when it came to the categories of biology, than to define life in equally functional terms But in a day unfamiliar with any other biological functions than those of nutrition and reproduction, it was inevitable that a natural philosopher of the Aristotelian school should frame his definition of life in terms of these two functions alone It should, however, be noted for future consideration that however abortive this classic attempt to define life in terms of these two functions alone might prove to be, it would not follow that *no functional definition of life is possible*

But whatever had been the historic origin of the experiment at life-defining now to be examined, the course of that examination must have been the same For, as usual, pure logic prescribes an exhaustive classification of the possibilities of the case They are four If life is to be defined in terms of nutrition (n) and reproduction (r), then we must accept one of the following propositions

All living and no non-living bodies share

   (i) the functions n and r,

   (ii) the function n but not r,

   (iii) the function r but not n,

   (iv) either one or the other of the two functions n, r

Of these four possibilities, (i) and (iii) can never have been seriously considered the knowledge that not all classes of living beings share in the function of reproduction must have been as old as man's acquaintance with hybrid forms [2]

Turning to the second possibility, it was long held, and may still be held by some, that to possess the function of nutrition is not only a necessary but a sufficient condition to constitute a body possessing it a member of the class *living bodies* Those who so hold must consider themselves able so to define nutrition as to make it a function common to all living, absent from all nonliving bodies If some have come to doubt whether biology has defined or can define nutrition in a way to make the nutritive function a common possession of all that lives, it is because certain comparatively recent observations, made on a class of bodies previously familiar enough under the name viruses, have thrown the biologist into uncertainty as to whether viruses are or are not to be included in the class of living organisms What these observations established need not be recited here, it is enough to remark what they did not establish They did not establish in viruses a nutritional process falling under any previous definition of that process as found in recognized forms of living being More than that observation of viruses, so far from establishing the existence in all viruses of a nutritional process analogous to the essentially meta-bolic nutrition of known organisms, placed difficulties in the way of admitting the possibility of a metabolic process taking

respectively, to the patterns of ontogenic and progenic functioning, suffice to define the concept of life?

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place in some viruses ³ And other processes previously supposed to be peculiar to organisms were definitely established as attributable to viruses The result in face of presumptive evidence that no nutrition dependent on that description of metabolic process which biology had previously absorbed into its very definition of nutrition was to be found in viruses, biologists still entertained the possibility that viruses were ultimately to be included in the class of living beings

This raises the question that interests us at the moment Does not biology entertain the possibility of so defining the life-class that membership in it may be established on grounds independent of evidence that a given class of living beings exhibits a nutritive functioning? If so, then of four possibilities in the way of defining life in terms of the functional properties nutrition and reproduction alone, only the fourth and last remains available It alone would include among the living, a class of bodies which (like the non-reproductive hybrids) exhibited nutrition alone, or a class which (like, it may be, the viruses) exhibited reproduction alone In a class of the living so defined the viruses would be secure of a place for if there is any doubt as to whether or not they exhibit a process properly to be called nutritive, there is no doubt that viruses are enormously reproductive

From this review, it may be concluded that of the four possibilities in the way of defining life in functional terms alone, the first three, so far from defining, are not even true of the class living being The fourth, to the effect that all living beings exhibit either the nutritive or the reproductive function, even if ultimately accepted as true, is still far from furnishing a definition of life How far, it will be well to consider at this point, by way of affording a preview of troubles that lie ahead

Or, rather, instead of confining our reflections to the

additional functions of nutrition and reproduction, let us consider the matter in terms of ontogenesis and progenesis that is, instead of dwelling on empirical examples of functions conforming to one or the other of two formal patterns of functioning, let us give our thought greater generality and clearer definition by turning to the formal patterns themselves What we ask, then, is this if it be true that all living bodies exhibit either an ontogenic or progenic function, is it equally true that no nonliving bodies function in either of these two ways? For, of course, both of these conditions must be fulfilled by any proposition which could serve as a definition of *life*

In the course of recent years, laboratory ingenuity has invented several types of demonstration apparatus designed to "mimic" the nutritive function of life These constructions, which we may loosely group together under the name 'artificial cells," do at least exhibit phenomena that conform to all the requirements imposed upon ontogenic functioning They grow, as organisms grow, by selective absorption of environmental material into the mass of the "cell", this absorption and growth depends on an intussusception and not an accretion of environmental material The ingested material then undergoes a process of chemical transformation which, whether or not we call it "metabolic" in whatever technical meaning a biologist would put into the term, is at least metabolic in a purely etymological sense, that is, it does result in assimilation of some, dissimilation and ultimate elimination of other parts of the substance ingested In no one of the various types of such artificial cell as have fallen under the eye of the present study, does this ontogenic functioning result in progenesis But this, of course, is irrelevant to our present argument, since the same is true of the nutritive functioning of nonreproducing

hybrids Yet while every one takes these hybrids to be alive, no one classes these artificial cells among living bodies

And what of the other of the two functions, one or the other of which is taken to be the property of every living being? Is the function of progenesis any more peculiar to the living than is the function of ontogenesis?

One may point to phenomena dependent on no laboratory contrivance but presenting themselves in the uncontrolled course of nature that meet all the requirements imposed on bodies exhibiting progenic functioning There is, for example, a quite plausible theory of, what we may call, the "birth, growth, reproduction" of a raindrop, which whether it cover the history of any drop falling to earth or only a period in a more complicated story, serves equally well to illustrate a phenomenon of progenic functioning dissociated from any evidence of ontogenic functioning

The story begins with the moment at which atmospheric moisture gathering about a solid "dust" particle forms an "embryonic raindrop." With continued accretion of moisture, this embryo attains a weight sufficient to overcome atmospheric resistance and begins to fall earthward Continued accretion of moisture about the falling drop increases its mass to the point at which the drop can no longer be withheld from dividing by the force of surface tension Under the atmospheric conditions that result in a terrestrial rainfall, each of the two "offspring" drops, into which the "parent" drop has divided, again increase in mass and again divide, continuing a story of "birth, growth, reproduction" whose characteristic episodes are repeated as many times as atmospheric conditions require before the progeny of the original progenitor of this progenic line of rain drops reaches, wets, the earth If, now, we let v_1,

x_2, x_n represent the structural state of each offspring drop just after the division of its parent, y_1, y_2, y_n, the state of each parent drop just before its division into offspring, we see how completely the x_1, y_1, x_n, y_n, sequence thus symbolized conforms with all the requirements imposed by Post [VIII] on a sequence of bodies exhibiting a progenic function In particular, we see how closely the genealogy of a raindrop falling on the earth mimics the like history of any species of unicellular organism in which reproduction is effected only by division

With this, we have learned all that is to be learned from that chapter of biological history which undertook to define life in terms of the two functions of ontogenesis (in the form of nutrition) and progenesis (in the form of reproduction) Of the four classes of body definable in terms of these two functions alone, not one includes all the bodies and none but the bodies which common opinion and scientific judgement take to be living What then is to be done, if life is not to be left that 'indefinable mystery' which the greater part of humanity has always taken and does still take it to be?

Logic, once more, classifies the possibilities of the case properties defining the class *living being* must be either (1) all of them non functional (physical), or (2) some of them nonfunctional, some functional, or, (3) all of them functional properties

Of these possibilities, the first is accepted by that school of natural philosophy now generally called *mechanistic* Of this school, Democritus (or perhaps his master Leucippus) is taken to be the founder ⁴ Among moderns, Descartes is the first to have offered a mechanistic interpretation of all subhuman life In human beings, however, he would have the pineal gland exercise a freedom to direct the course of

"rational" behavior, in violation of the laws of mechanism. After Descartes, a mechanistic biology, purged of this human faculty of a mechanism violating freedom, is plentifully represented in a history whose story has frequently been told.

Of the second possibility, contemporary biological discussion has availed itself in several ways. Among the nonfunctional requirements taken by one or another analyst as conditions differentiating the ontogenic and progenic functioning of living bodies from the like functioning of the nonliving may be listed:

(a) Physicochemical conditions laid upon the structures whose functioning is in question; e.g., the requirement that the structure of living bodies shall include protoplasmic constituents.

(b) Physico-chemical conditions laid upon the process by which this functioning is effected: e.g., the requirement that the ontogenic functioning of living bodies shall take the form of a nutritive process involving physico-chemical transformations, not reproduced in the like functioning of nonliving bodies (artificial cells, etc.).

(c) Nonfunctional reactions, empirically recognizable, though not yet definable in physico-chemical terms; e.g., those attributed to the "irritability" of living matter.

Once more, the present study allows itself a major economy. It rejects, without analyzing its reasons for rejecting, all attempts to differentiate living from nonliving bodies in terms of nonfunctional properties attributable to the former and not reproduced or reproducible in the latter. To one who has in mind the literature of life-defining, this major economy will be taken for a major omission. It is so; and only because it is so does a review of the vast and often highly technical literature omitted become as unnecessary

as it is impossible of summary accomplishment If all that is
to be said for and against any one of the theories in question
has not already been said, at least the present discussion has
nothing enlightening to add to the debate Any one whose
mind is not yet made up on the matter at issue may read
and reflect for himself, the literature is before him As for
us, our conclusion has been unequivocally stated It bears an
implication which we willingly accept, the implication,
namely, that were we limited to the ways above listed of
differentiating the living from the nonliving by attributing
to bodies of the former class nonfunctional (physico-
chemical) properties never to be reproduced in bodies of the
latter class, we should share the skepticism of more than one
biologist as to the possibility of finding a connotation for a
term whose denotation is in large part, at least, so definitely
known as that of life "In many branches of biology (writes
a specialist in that field) exact definitions are difficult
Winterstein states, 'Whoever tries to develop general
conceptions concerning the course of vital phenomena, is
ever and again brought face to face with the painful but
irrefutable fact, that the very concepts which are most often
used lack essential clarity and unity of meaning'   It is
characteristic of scientific workers, in contrast to philo-
sophers, that they are eager to go forward and study
concepts without taking the time or effort needed to
define them This is no doubt wise, for as work develops
and the knowledge expands, the concepts tend to become
clarified "[5]

Our study, then, has brought us to this conclusion, or at
least to this decision (1) To abandon the classic hope of
defining life in terms of the functional properties of onto-
genesis and progenesis alone the class of bodies exhibiting
both functions is too narrow to include all life, the class

exhibiting one or the other is too broad to exclude all non-life. (2) To abandon the still persistent attempt to differentiate the ontogenic and progenic functioning of the living things from the like functioning of nonliving things in terms of a difference between the materials that function, or between the machinery of the functioning. There remains but one possibility open in the way of a differentia that shall mark the living from the nonliving; we must find at least one functional property, other than those of ontogenesis and progenesis, which shall be shared by all living and by no nonliving bodies. But in what direction may we look to find any such new property or properties? Let the question set the topic for the next chapter.

¹Editorial Note: "Things" may refer to the states of one body or the states of separate bodies; see [VII] and [VIII] below

²It is true that Aristotle sometimes speaks of nutrition *and* reproduction as though both were universal to life. (E g. *De Anima*, 432b, "The movement of generation and decay, being common to all living things, must be attributed to the faculty of reproduction and nutrition.") But, to bring such passages into harmony with what he elsewhere says, one has to interpret the term "common" to mean "holding for the most part" rather than "holding always " (cf *ibid*, 415a, "The acts in which it [the nutritive soul] manifests itself are reproduction and the use of food Reproduction, I say, because for every living being that has reached its normal development and which is unmutilated, and whose mode of reproduction is not spontaneous, the most *natural* act is the production of another like itself " (Ital ours)

³The experimental work of Elford, begun in 1930, established more accurately the limits within which the size of various types of virus ranges "(Elford) demonstrated . . that whereas some viruses were as large as 300 mu (millimicrons) others were as small as 10 mu. It was soon realized that the acceptance of a virus 10 mu in size as an ordinary living organism presented certain inherent difficulties, especially with respect to metabolism Grave doubts were expressed that the complicated processes of respiration and digestion and other metabolic functions

of life could be contained within structures as small as 10 mu, especially since protein molecules larger than 10 mu were known " Stanley, *American Scientist*, Jan , 1948, p 60

⁴Of the vast range of Democritus' writings, nothing remains Our understanding of him has to depend on quotations from, and what purport to be reproductions of his thought With all we know from these secondary sources, one may consistently take him to have been a thoroughgoing mechanist If other interpretation of the historical data is permissible, this is not the place to examine into the matter (All available source material is to be found translated in *Selections from Early Greek Philosophy*, Ed M C Nahm, 3rd ed , N Y , 1947)

⁵Heilbreunn, *Outline of General Physiology*, Philadelphia and London, 1938 pp 399, 402

exhibiting one or the other is too broad to exclude all non-life. (2) To abandon the still persistent attempt to differentiate the ontogenic and progenic functioning of the living things from the like functioning of nonliving things in terms of a difference between the materials that function, or between the machinery of the functioning. There remains but one possibility open in the way of a differentia that shall mark the living from the nonliving; we must find at least one functional property, other than those of ontogenesis and progenesis, which shall be shared by all living and by no nonliving bodies. But in what direction may we look to find any such new property or properties? Let the question set the topic for the next chapter.

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22. Post-Darwinian Postulates

THE TURN OF PAGE THAT CARRIES US FROM THE PRE-
Darwinian to the Post-Darwinian period of history brings
into view a world in which *life* plays a new part and takes on
a new meaning So, at least, the present study interprets
this moment of history and what follows on it, but there is
more than one way of telling the story It may well be that
the general reader, reflecting on the change wrought in our
science by the introduction of the evolutionary theory of
life, will have recognized the great importance of this
change without having realized to the full the depth and
far-reaching nature of the import our study accords it
Certainly, the new biology had brought within range of our
imagining the solution of a problem that previously had
staggered all conjecture The cosmologist had so far
advanced in reconstructing the course of the solar system's
development as to make it clear that no form of living being
that has been known to man since man began to know life
could have existed in that system before a certain date
Even before the possibility of "spontaneous generation" had
been definitely excluded as accounting for the recent origin
of any form of life, it had been impossible to imagine that all
the vast variety of living creatures including man could
have come into being thus "spontaneously " But to picture
some type of organism, scarcely differing in its bodily make-
up from certain types of inorganic body, coming into being
out of a previously lifeless medium—this was so far from
taxing our powers of conception that the thought of such a

birth of life had occurred to several of the Pre-Socratic Greeks Then came the thought that from the simple had evolved the complex, and with Darwin's carefully gathered evidence that what could have been had been, our minds were not far from being at rest

But while all this results in a picture of life's history vastly different from any that Aristotle could have accepted, in what way does it change the *meaning* that Aristotle on the one hand, Democritus on the other, attributed to life? Does it add a third function to the nutritive and reproductive functions on which Aristotle depended for such definition as he could formulate of the class living being? Does it change in any way the meaning of either nutrition or reproduction as Aristotle understood these terms? Or, failing any contribution evolutionary theory makes to the Aristotelian conception of organic functioning, does the great progress made through the millennia following Aristotle in our understanding of the physical structure of living bodies and the physics and chemistry of the vital process that keep *these bodies "going"* bring us nearer than was Democritus to defining life in terms devoid of functional implication? In short, how does evolutionary theory change the meaning of life as it was understood in Pre-Darwinian days, or, rather, how does this theory bring us in better hope of *giving* a meaning to *life*—a meaning which, as the brief historic survey of the last chapter showed, Pre-Darwinian biology had been unable to frame? That is the question to which this and the following chapter devote their thought

Continuing in the method our study has followed from the beginning, we will set down in their final wording the postulates to which we must adjust our observations of Nature, if we are to fashion a concept of life that will serve as an instrument to advance us toward the objective that

has long been before us; the objective, namely, of making that embodiment of science, the scientific man, at home in the physical universe that he himself has patterned.

The task before us is difficult and complex. To make our workmanship the easier, we will introduce such extensions of the symbolism already brought into play as will make our formulations more compact and easier to keep in mind. Listing these conventions in the subjoined footnote,[1] we turn at once to the formulation of our postulate.

Postulate VI[2]

Let there be a manifold

(1) of *bodies*, each body composed of a continuous sequence of *states* $s_i]_1^k$, of which each state has a *physical morphology* such that the morphology specific to each state conforms with a law holding for them all;

(2) of *links*, each link composed of a sequence of bodies $b_i]_1^m$, such that a morphological property of each antecedent body is *reproduced* in a like property of each subsequent body; and

(3) of *chains*, $c_1, c_2 \ldots$, each chain composed of a sequence of links, such that a morphological property of each antecedent link is recapitulated in a like property of each subsequent link; and

(4) of *lines*, $L_1, L_2 \ldots L_m$, each line composed of a sequence of bodies $o_i]_1^n$, of which each subsequent body is *born of* its antecedent body and has a morphological property that *reproduces* the like property of some antecedent body, and such that the sequence $o_i]_1^n$ includes all bodies composing a single chain c; and let

(5) c and c' be two chains of incompatible morphology, in each of which k, m, n are greater than 3;

then and only then

(a) all bodies composing links will have an *ontogenic morphology*,

(b) all links composing chains will have a *progenic morphology*,

(c) all chains included in lines will have a *phylogenic morphology*,

(d) all bodies composing lines will constitute a *biogenic sequence* in which each antecedent stands to its (immediately) subsequent body in the (reciprocal, intransitive) relation of *parent to offspring*,

(e) the last bodily states of the producer-product sequence of states composing the chains c and c' respectively, will have an *ontogenic, progenic, phylogenic function*,

(f) all bodies composing a biogenic line and only such bodies will be *living bodies* or *organisms* [22]

Comment

It will be seen that the brackets enclosing the postulate-numerals of the last chapter have been allowed to stand and that the Postulate VI of the present chapter is to be understood as following immediately on the Postulate V formulated in Chapter 19 This means, as was explained in introducing the "tentative" postulates with their bracketed numerals, that the experiment for which they were to provide the apparatus has failed But this does not mean that anything said in the course of the argument in which that experiment was tried out, is now repudiated as something false, rather, it has been discarded as something inadequate to the purpose for which it had been designed No doubt empirical data could be adjusted to the formal demands of these tentative postulates, but with all adaptations made, the result *falls short of providing us with a connotation of the term life* that would accommodate all the

bodies and only those bodies which the biologist of today, or indeed the plain man of the ages, would number among the living In short, the knowledge of life which these tentative postulates attempted to formalize has been outlived, and if we are to succeed in doing what this attempt failed to do, "we must," as Plato would have it, "make a new start"

The course this new discussion must take is shaped by the grammatical structure of our new postulates a convertible conditional sentence, the postulate offers the propositions of its consequent clause as true, *if and only if* all the proposed conditions of its antecedent are simultaneously met Then and only then will we be able to define a class of bodies that we, largely guided by the biologist of our day, are willing to class among the *living* The development of our thought on the meaning of *life* will be most easily followed if we present in succession propositions that purport to be simple statements of fact, then examine the empirical evidence adduced in support of each of these propositions, finally inquire whether the class of bodies of which assertions are made includes all bodies and only those bodies we are willing to call *living*

(A) As the first of the propositions that we offer as empirical findings, we set down the following There are to be found in Nature bodies whose respective life courses are composed of a continuous producer-product sequence of states, such that a physical morphology specific to each state conforms with a law holding for them all

To adduce the empirical evidence supporting this proposition it will be well to begin with as clear a picture as can be drawn of what is meant by the law to which the course of an organism's life is to conform Any formulation of this law must presuppose a definition of a term that enters into its conditioning The definition has only to be recalled,

since it was formally worded in Chapter 16 of the present study, but for ready reference the definition is here repeated with, however, a slight change of wording which allows a clearer picture of the empirical application of the term

Physical morphology A manifold of structures having in common at least one physical property, so quantitatively distributed among individuals as to make them all approximate to within definite limits a physical norm, constitute a class of structures having a common *physical morphology*

How the quantities in question are to be "so distributed" among the members of the class as to warrant the application to them of that theorem of probability theory by which we may calculate the norm to which they approximate, together with the standard deviation that measures the closeness of the approximation, has been so fully discussed in other texts as to need no amplification here

Now it will readily be granted that every living body meets the condition imposed by VI (1) in so far as its life course is composed of a continuous producer-product sequence of states, each having a physical morphology, but does every living body meet the further requirement that the physical morphology specific to each state of this sequence conforms to a law holding for them all? The hypothesis that it does meet this condition may be most clearly expressed in simple algebraic symbolism

Let $s_i]_1^k$ represent any continuous sequence of states of which s is a state of a given physical morphology through which the body passes at a date t_i indicated by its subscript, at which date the age of the organism will be $(t_i - t_1)$ Then the requirement that the states of this sequence conform to a single law, may be expressed in the form of the equation $s_i = f(s_1, t_i)$, i e, the physical morphology of the organism's state at age $(t_i = t_1)$ will be a function of (dependent

on) the like morphology of its initial state s_1 and of its age
But this equation, while expressive, is not exhaustive of the
meaning of producer-product relation existing between the
earlier and the later states of an organism's life course To
complete that meaning, we turn back to an earlier page of
the present study, to the Note, namely, appended to Chapter
18 There it was shown that if x_i is a producer of y_i, $\bar{x}_i$ is
a co-producer of $\bar{y}_i$, where X and $\bar{X}$ are physico-morphological
classes to which x_i and $\bar{x}_i$, respectively, belong We may pass
over the details by which one applying this general principle
to the special case of an organism's life course conforming to
law, would be led to express this requirement in the form
of the equation $s_i = f(s_1, \bar{s}_i, t_i)$, i e, the physical morpho-
logy of an organism's bodily state at age $(t_i - t_1)$ is a function
of (now in the sense of determined by) (1) the physical
morphology of its initial state s_1, (2) the environment $\bar{s}_i$
correlated with each state s_i, (3) the organism's age at date t_i

Such an over-simplified expression for the life course of a
body conforming to the requirements of VI (1) does not, of
course, pretend to be a summary account of what the
laboratory has experimentally established It is offered
merely as giving a clearer picture than could otherwise be
drawn for what the experimenter is seeking in the way of
what we may call an organism's way of life We turn from
the formal pattern of a body whose life course conforms to
the requirements of VI (1) to its empirical application How
far can such experimental data as the biologist has gathered
from his observation of organisms be adjusted to this formal
pattern?

In the sequence of environmental states, $\bar{s}$, correlated
with the sequence of the bodily states, s_i, we recognize
what is commonly called the *regime* to which the body has
been subjected When the body is an organism, the physico-

morphological class to which any state of this regime is assigned depends upon the measure to which it contributes to the "nourishment" and "growth" of the organism, that is to its respiration, alimentation, hydration, etc Since the actual sequence of states through which a body passes in the course of its life varies with the regime, some one life course must be chosen as standard to which others may be related as departures from standard

The layman of today comes into contact with the bio-sciences in too many ways to be left in any doubt as to what the scientist's choice of standard is Whether from his general reading, or from the counsel of specialists whom he has consulted in concern for his own life or the lives of others, the layman becomes thoroughly familiar with the sound of two correlative adjectives *normal* and *abnormal* He hears of normal and disturbed metabolism, normal, retarded, excessive development of organ or organism, normal or unfavorable environmental conditions, etc He comes to understand something of the physician's three problems (1) a diagnosis, by which the practitioner would come by what knowledge he could of two bodily states a normal and an abnormal, and the direction and degree in which *the latter departed from the former*, (2) a thera peusis, whereby from what knowledge his science can gather of the conditions that produce this abnormality, he might judge whether counter measures would reduce it, (3) a prognosis, of which among the many forecasts the word *suggests*, that of "life and death" importance to the patient is whether the abnormality is judged to be curable or incurable In the end, the layman is duly impressed with the importance to science of those two correlated terms, but what does the scientist himself mean by the normal and abnormal?

The question would be easier to answer than it is, were it not for an ambiguity in the sense in which scientists no less than laymen use the term *normal* Consider these examples A physician reports the condition of a patient whom he has just examined to be *normal* An anthropologist reports an excess in stature of northern over southern European to be *normal* Are these two scientists using *normal* in the same sense? So far from it that the relation between the physician's and the anthropologist's use of *normal* is one of contrast rather than resemblance The percentage of a population whose condition approximates the physician's norm *decreases* the closer the approximation, the percentage whose stature approximates the anthropologist's norm *increases* the closer the approximation

To avoid confusion arising out of this double sense in which *normal* is used, and correctly used, we shall refer to the anthropologist's *normal man* as the *average man*, reserving the term *normal* to the use of the physician We are duly impressed with the importance to the bio-scientist of the terms normal and abnormal but what does he mean by the normal?

It may be easier to set the conditions to be met by an organism whose life shall run a normal course, than to devise an experimental method by which to determine how near to, or how far from the life so defined a given course may have run By way of conditions we may say the life of a given body, an organism, will have run a normal course if and only if at each moment of its existence that organism will have enjoyed a higher life expectancy than it would have enjoyed had it followed any other course Since, now, the life course of an organism is determined by the equation $s_i = f(s_1, \bar{s}_i, t_i)$, and since the initial state, s_1, of that course is a fixed parameter beyond experimental control, it follows

that the only experimental problem set, is to prescribe the regime which would insure a life course that would afford the organism at each moment the maximum life expectancy possible to it Such a regime may well be characterized by the single adjective, *optimum*

So we may define the meaning of *normal* But how design that optimum regime which would assure an organism a normal course? The question falls under two heads First, to determine the range of courses the organism might run as correlated with variations of the regime, second, to determine which of these courses would be correlated with maximum longevity The answering of these two questions *seems to the layman to present difficulties beyond hope of solution* If anything were needed to give point to the layman's apprehensions, consider the first question How to determine experimentally the range of courses the life of a given organism could run, each course being correlated with a given regime? Here obtrudes itself with particular insistence a problem that has been with us ever since the conditioning of producer-product relations called for our study how to obtain experimental support for a hypothetical proposition whose antecedent was a condition contrary to fact? How, in attempting to solve the problem of a normal life, to determine the course that life would have run, had it been subjected to a regime other than that to which it was actually subjected? How, if that question finds an answer, to proceed to judge which of these courses would have had the longest run?

But we pursue the topic no further We know that to generation after generation of the ablest minds, gathering science as they went, these questions have not seemed unanswerable We accept on faith, the faith of the scientist, that he is right, *and our faith is rewarded by the benefits*

we enjoy by acting on the assumption that he is so: the benefit to the patient, who consults his physician; to the breeder, who consults his veterinarian; to the planter, who consults his agronomist.

Such then is the relation between the actual and the normal life course of a given organism and our reason for having faith that these difficulties are not insuperable to the experimental scientist. But is there a word in all these statements which we would change were we to substitute for organism, any implement of the arts? Would its normal course not have exactly the same meaning, and would the diagnosis of its actual course the same difficulties, and should we not have the same faith in the technologist that we had in the bioscientist—faith that these difficulties were not insuperable? In what way, then, differentiate a normal life course of an organism from a normal "life course" of any inanimate object? Is there any condition general and peculiar to the norms to which the lives of organisms may conform, which inorganic bodies never meet? The opinion of the present study, so often expressed in the past, that no such condition is to be found gives us a sense of relief in observing that our Postulate VI (1) makes no reference to any such differentiating conditions. It does indeed require that the succession of states composing the life of an organism conform to a law, but it makes no reference to any *particular law nor do we consider it possible to find any such* condition as would differentiate the living from the non-living bodies. Our own course of differentiation is specified in the remaining requirements of (VI)—requirements that our postulate proposes must all be fulfilled before any one of them is to be interpreted as applicable to the organism and not to the inorganic body.

It is sometimes possible, and when possible generally

rewarding, to take a glance ahead and seize the more obvious course that lies before one, quite understanding that its windings and turnings must await closer exploration and mapping Such a glance ventured at this point would suggest *the following conclusion to our argument* The definition of *life* is not to be framed in terms of a condition common and peculiar to all the norms to which the normal lives of organisms may conform It is to be framed in terms of the laws of heredity which are indeed common and peculiar to the organism and sequence of organisms composing an evolutionary line

The preceding discussion puts us in position to give formal definition to the concept with which its final paragraphs deal, and which will find immediate application in the comment that follows

Ontogenic morphology To an organism whose normal life course is composed of a producer-product sequence of states such that a *physical morphology* specific to each state conforms to a law holding for them all, is to be attributed an *ontogenic morphology*

With this definition we recall the last of those conventions made in the interest of economy that prepared the way for the wording of (VI) and of all discussion that was to follow That convention would have it understood that a morphological property attributed *to all collectively* of a producer-product sequence of states was to be attributed to *all these states distributively* Accordingly in the present instance, to all distributively of those states to which collectively we have attributed an ontogenic morphology we are now to attribute distributively a *like ontogenic morphology* It will be understood that this result is not a mere convention but a logical deduction from our premises For although the states composing this sequence differ in, say,

their physical morphology, yet their variation is subject to a single law The property, therefore, of all, distributively, of these states conforming to this law is shared by them all and with it the ontogenic morphology just attributed to them The importance of this common attribution will appear later when we come to discuss the functional properties of organisms

To introduce our comment on the second requirement of our postulate, we offer as another simple statement of fact the following

(B) There are to be found in Nature producer-product sequences of bodies such that a morphological property of each antecedent body is *reproduced* in a like property of each subsequent body

The newer (post-Darwinian) biology would doubtless agree with the older (pre-Darwinian) biology that some but not all organisms were thus reproductive, but would the newer also agree with what would doubtless be the opinion of the older and with the common view that only organisms were thus reproductive?

We may give this question sharper point and deeper perspective by wording more historically its import We take it, then, that there was a time when all thinking men would have with one accord held the opinion that only living bodies could compose a producer-product sequence of which each successive member reproduced the ontogenic morphology of the antecedent member And that may well be the general sense today, but will that general sense be any longer unanimous? Will there not be room for those who learn from Darwin's teaching a double lesson, first, recognizing as does all the world that we have here the first acceptable theory as to how life happened and is happening,

and second, to have considered for ourselves whether in view of Darwin's results we should not reform our definition of what life is

To begin thinking our way toward an answer to this question, we introduce in simple form a producer-product sequence that with certain added conditions and under another name will be considered again in a future comment For present use, a *parental line* will be a producer-product sequence of organisms such that each antecedent term will stand to its immediate successor in the (reciprocal, intransitive) relation of parent to offspring and any antecedent to any successor in a (reciprocal, transitive) relation of ancestor to descendant We take up our story at the moment when two millennia of pre evolutionary theory lies behind us and a scant century of evolutionary theory lies before us As the earlier period draws toward its end, we find the biologist preoccupied with *the problem of the classification of organisms, and by the time* that the boundary line is reached we have before us those various systems of taxonomy that the later evolutionary theory called artificial But whether old or new the classification of life forms has always a common problem before it to gather organisms into mutually exclusive and collectively exhaustive species covering the domain of life The species must be *infima species* as biology understands the term, but how the *infima is to be defined is a part of the particular system* which applies it

To meet this condition the classifier must select certain properties generic to all organisms but susceptible of varying specifically from organism to organism Such indeed is the problem the biologist who classifies the manifold forms of life must face whatever *the period of his thought*, but for the moment of our thought it will be apparent that this

task would be beyond all performing were organisms
entirely independent of one another in the manner of their
production The extent to which they are not is caught up
in the single sentence, "Every organism is one of the bodies
composing a parental sequence of bodies," or in its equiva-
lent sentence, "Every organism is either ancestor or
descendent of another organism " (It is of some historic
interest to note that in this last sentence the "either/or"
saves the primal ancestor of any parental or ancestral line
from being swept into the *omne vivum e vivo* of an earlier
school) Considering now the conditions to be met by two
bodies of which one shall be parent to the other we find the
primary requirement to be that the parent shall have pro-
duced the offspring by giving birth to or begetting it Here
it may well be assumed that evidence supporting the pro-
duction of one organism by another through birth or
begetting could be established on empirical evidence quite
independent of any resemblance between the two Such
evidence as might justify our calling the offspring a repro-
duction of the parent would have to be gathered from a
statistical study of an array of pairs of parents with offspring
which are given to us by definition That is, the maximum
manifold of possible examples that we might survey would
consist of all members existing between those of some
remote age and those existing today Now, from his obser-
vation of such samples of this boundless array as might be
available to him the biologist of the old school could have
drawn but one conclusion, namely, that all sample sub-
sequences of equal length from whatever part of the
parental line they were taken varied in their morphology
by no more than a limited amount from some common
mean Assuming that the conditions holding for these
samples of a parental line held for the whole, it would be

logically possible, whether or not empirically profitable, to design a scheme of classification of the following kind

Let us gather into one class all parental lines meeting the condition that the components of any two classes should differ no more in their average morphology from each other than did the components of any one class [3]

The empirical question would then be whether organisms could be gathered into classes so designed that the classes formed would be mutually exclusive and collectively exhaustive of the realm of organisms If so, and only if so, would these classes meet the conditions imposed upon what the biologist means by a species But will further observation confirm the hypothesis that what holds for this sample of organic life holds for the entire domain?

We have no occasion to inquire further into the troubles of the old school biology We have come to the promised moment when the line could be sharply drawn that separated the old from the new biology The decisive word has been spoken It lies in the premise, classification of life forms into species must be so designed that offspring of parents of the same species shall be of the same species as the parents In the denial of this premise all would recognize the key that opens the door to the *Origin of Species* and therewith to the whole evolutionary theory of life that follows

Here we interrupt ourselves for a moment before examining what lies on the other side of the dividing line we have just drawn, to take what should be an unnecessary, but may be a wise precaution There will be those who in their fidelity to history will ask on what page of what author may we find the premises which you have assumed the old school of biology to have accepted as its postulate? I answer on none From the beginning we have made it frequently explicit and everywhere implicit that we were looking over

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the shoulder of the experimental scientist as he worked in his shop, observing what he did but not attempting to record what he said Everywhere it is understood that the proposals which we accept as postulates are propositions to which the thought and actions of a certain school may be adjusted, not sentences or propositions that members of that school have formulated In what belongs to the present chapter we have tried to formulate those proposals to which the biological knowledge of today may be adjusted And now we turn to the first of those proposals

As a focal point from which lines of contrast may be methodically developed, consider the respective attitudes of the two periods toward a parental line For both periods, a parental line is composed of a producer-product sequence of organisms of which each antecedent term produces its immediately subsequent term by a process called "giving birth to" or "begetting," whereby the producer acquires the relation of parent-to-offspring to his immediate product, or of ancestor-to-descendant Having established such a line on empirical evidence, the old school found inductively that the terms of this producer-product sequence were sufficiently similar in their ontogenic morphology to justify us in speaking of the morphology of the offspring as a reproduction of the parent

But now, for the new school, if one assigns to one biological species all those lines in which the morphology throughout the sequence remains constant and assigns to two different species those in which the morphology differed, in the end on the basis of this classification the biologist would also be justified in saying that parent-offspring or ancestor and descendant were always members of the same biological species These subsequences our postulator has called "links" and to any two terms of a single link of which

the latter is a reproduction of the earlier, he accords the name progenitor and progeny. So the subsequence of terms which we have called a link we may now refer to as a *progenic link*.[4]

[1]Letters x, y,   continue to represent morphological classes of which x_i, y_i,   are individual members (see p 262) The species of morphological class for which these symbols stand will no longer be restricted, as in the last chapter, to the physicomorphological class defined in Chapter 17, but will be generalized to include any one of the species of morphological class conditioned in Postulate IX To economize symbols, classes represented by different letters, x, y,   will be understood to be of incompatible morphologies, i e , x and y will be abbreviations of the symbols xy , x y (x and not—y, not—x and y)

The symbol x_1^n will represent a sequence $x_1 \ x_2 \qquad x_{n-1}, \ x_n,$ of which each antecedent stands to each subsequent term in a producer product relation

Propositions attributing a morphological property to all terms collectively of a sequence x_i^n will be understood to attribute the same morphological property to all distributively of the terms composing that sequence

[2]Editorial Note See immediately following under *Comment* why this Postulate is so numbered

[2a]Editorial Note Because in the sequel the comments on this Postulate may become tedious and sometimes confusing, it may be worth while attempting a brief restatement Students of Singer will recognize the discussion of this and the next chapter to be a discription of what he called the "Cone of Life" in his graduate seminars at the University of Pennsylvania A very excellent summary of this earlier version of Singer's thinking is to be found at the end of E F Flower's "Two Applications of Logic to Biology" in *Philosophical Essays in Honor of E A Singer, Jr* (Phila , Pa , 1942) In the present version Singer has gone into a great deal more detail concerning the patterns of development in bodies, links and lines than he had earlier Living bodies must first of all have three morphologically defined properties Their growth must proceed through a series of stages, such that all stages comply with a common law Next, we can group together a set of bodies, much as was done in earlier (pre-Darwinian) biology But earlier biology was content

to make the grouping in terms of a common set of properties Later
biology makes the grouping in terms of a very refined concept of repro
duction The growth of any body can be divided into two parts the
protogenic is a common form of all the species, the metagenic differs
among members of the species according to genetic ("Mendelian") laws
The bodies of a common link agree in the patterns of their growth the
protogenic properties and the laws governing the metagenic period are
reproduced in body after body in the link We now think of the links as
joined together in such a fashion that the links recapitulate each other
and a set of such links are called a chain We add the further condition
on the bodies that each is "born of" its antecedent in the link, where
"born of" must be given a technical definition (see p 378) When this
occurs the chain becomes a line We add the condition that the number
of elements of a chain must be sufficiently large so that we can ascribe to
them the potential production of a certain morphology Finally, by
imposing the condition that there exist chains of unlike morphology we
can ascribe functions to the bodies composing the chain The Postulate
then states that all bodies composing such biogenic lines are living, and
all living bodies belong to such biogenic lines

As further examples of the need for so elaborate a model of the
organic world, consider the following

   (a) A bird's nest built of inanimate material is not living Yet it is
   a producer of the egg just as much as the parent birds One might
   indeed find a common law governing the growth of every nest, and
   even a common progenic morphology for nests of birds of a given
   species Evidently the nest fails to be included because the nest was
   never a part of the body of the parent, nor the offspring a part of the
   body of the nest (i e , the nest is not "born of" the parents nor the
   young bird "born of" the nest)

   (b) The world of industrial products is strikingly similar in its
   structure to the world of organisms, especially if we include man in
   both worlds There can be no doubt of the evolution of industrial
   machines, and it might be fruitful to write a history of modern
   industry in terms of ontogenic, progenic and phylogenic laws
   Again, only the biogenic condition of having each item stand in
   relation of parent to offspring seems to fail in the evolution of indus
   trial machines

The reader should bear in mind Singer's general philosophical position

The aim is to find a model of the organic world that best suits the needs of the biologist, and *not* to find what is common to all living things (see for example Singer's exclusion of the completely stable "organisms") Singer's work is an attempt to formulate *what the biologist intends to investigate*, not what in fact people called biologists have from time to time investigated

⁸Editorial Note Apparently this stipulation means that there is no way to split the class in time so that there is a significant difference between the elements of the two classes resulting from the split

⁴Editorial Note It is clear that the author did not intend to end this chapter here, because the thought of the next chapter carries on directly from this point towards an answer to the question posed after (B) above, p 574 But the remaining material of the text was in very rough form, having been transcribed from a tape Therefore it seems desirable to group this material into the final chapter, where the editor has taken liberties in making the final revisions

23. Post-Darwinian Postulates (cont.)

IN THE MATTER OF OUR PRESENT CONCERN—THAT OF comparing the senses in which the term reproduction was used in each of two historic contexts—the results suggest both contrast and harmony; contrast in that for the older period to pronounce offspring a reproduction of parent was, in Kantian terms, a synthetic judgment; for the new school, to pronounce progeny reproduction of progenitor, was an analytic judgment. If the ancestor-descendant relation can be established, no question is raised as to whether any resemblance exists between the latter and the former—but not so the progenitor-progeny relation. Here we are to find an ancestor that so resembles its descendant that the latter may be said to be a reproduction of the former, where by reproduction we mean a product that resembles its producer in exactly that measure that is implied by the meaning Postulate VI (2) put into this term. What is this meaning? What measure of resemblance does our postulate understand its term "reproduction" to connote? Where if anywhere in nature are we to find organisms of which the producer stands to its product, the ancestor to its descendant, in this exact measure of resemblance? Let us divide the entire ontogenic sequence of states through which an organism is to pass in the course of its normal lifetime into an initial and final period, to be called respectively the protogenic and the metagenic period.[1] Throughout the first of these periods the morphology (let us call it a protogenic morphology) of the organism remains constant. That

is to say, the bodily state of all bodies composing a progenic line belongs in the same physical morphological class if in the same order of development In the metagenic period, however, the morphology of the organism varies widely from this strictness of uniformity Its variations, however, are not lawless, but subjected to a law of probability of which we must now give some account

All men, from the earliest human down to the latest pre-Darwinian, must have considered it certain that all the ancestors of a cat must have been cats, of a dog, dogs, of a man, men Although the post-Darwinian denied the universality of such ancestry, he would recognize that for prodigiously long subsequences of the bodies constituting a parental line, the proposition did hold that the morphology of each subsequent member reproduced that of each antecedent member in the sense of reproduction defined by VI (2), a sense which we have so far reconstructed as to recognize that throughout this subsequence the protogenic morphology of all members must be constant Here, then, we have a sequence which must by definition constitute a progenic line, but while the definition is a matter of human invention, the finding in nature of bodies which meet the requirements of this definition, is not. So then we reproduce the opinions of all pre-Darwinian thought by simply replacing the words "paternal line" with the words "progenic line " For a progenic line, it is indeed true that all progenitors of a given organism must be of the same biological species as the organism is

Turning now from those progenic traits whose inheritance by progeny is certain, to those whose inheritance is uncertain, we find this uncertainty not altogether uncontrolled Even the earliest men must have realized that some of these traits were more likely to be inherited than

others. That old wise science can have been of no very recent origin on the strength of whose teachings the gossip of today is able from time to time to predict properties of expected offspring with more success than failure. It would be within the gathered wisdom of the midwives' profession to have discovered that the offspring of two parents, one blue-eyed and the other brown, would be blue, the dominant trait. Again, the offspring of two parents equally blue-eyed, would be more likely to be blue than inherit the brown eye of grandparent or remoter ancestor. Such throwbacks grow rarer with the remoteness of the ancestor and come to be looked upon as marvels if not as portents. But what neither old wise science nor the best biography of the day could do some seventy years ago was to weigh these probabilities; not to say "more probable," but to say "how much more probable."

In 1865 Mendel reported results of an experiment in which he had crossed numerous varieties, previously established, of a single vegetable species, the pea, and recorded the number of offspring that resembled a given remoteness of ancestry in one or another of several selected attributes. Repeating the experiment a number of times sufficient to lend weight to its result, he found the numbers to be constant enough to permit of their expression as proportions, finally of proportions expressible in the scale of probability. Thus he would be able to say under like conditions how much more probable was the inheritance of a dominant than of a recessive attribute; again under like conditions, how much more probable was the inheritance of a near-lying ancestor than of a more remote one. These laws are to be found in the pages of any biological text, particularly in one that devotes itself to the problems of heredity. There they will be found to be of a complexity that puts them far

beyond the reach of the present study's possibility of reproduction even in outline. No simple sentence could convey the intricacies of the method by which Mendelian probabilities are calculated, nor the range of the possible uses to which, when calculated, they could be put. But perhaps for the moment, it will serve as a note on which we may hang a memory and forecast a vision, to word the following: by the data gathered and experimentally studied in the manner designed by Mendel, it is possible to calculate the probability that any given morphological trait will be shared by any two given members of a progenic sequence.

It might seem, then, that the moment had come for summarizing our conclusions as to the meaning of the term "reproduced" in the sense in which it is used in VI (2), and therewith of a progenic sequence which depends on that meaning. But this cannot yet be done for the reason that Mendelian variations and the laws of probability to which they are subject, are not universal to all forms of life. They are indeed universal to all such forms as propagate sexually, but only to those forms. They are obviously inapplicable to that extremely minute section of the whole domain of organisms in which each offspring has but a single parent, that is to say, in those very elementary forms that propagate asexually. In such forms, the total ontogenesis of a normal life course is not meaningfully divisible into a protogenic and a metagenic period. At least the latter statement is true, unless we make use of a device invented by the formal logician to ease the generalizations of the empirical scientist. This convention allows us to enter proper conditions which may be fulfilled vacuously. Thus we may say that in the organisms that propagate sexually, the protogenic period and the metagenic period are actually present. But in the organisms that propagate asexually the whole ontogenic

course conforms to the requirements developed, in our
definition, for a protogenic period All of the metagenic that
exists, namely none, conform to all the Mendelian laws that
apply, namely none And now we may indeed sum up We
find by this examination what Postulate VI (2) means by
the term "reproduction" Our summary may take the form
of the following definition

Progenic line a producer-product sequence of organisms
in which the morphology of the protogenic period is invari-
ant and that of the metagenic period in conformity with
Mendelian law, constitutes a progenic sequence or line, and
the law to which all members of the sequence conform, is to
be called the progenic law

And, now having interpreted in terms of formal defini-
tions the expressions "progenesis" and "reproduction" that
appear in VI, we may return to a full understanding of
what the question means, to the question raised at the
beginning of the present comment (p 574) However
different may be the understanding of life traditional to
pre-Darwinian days, and prevalent in post-Darwinian days,
our postulator would agree with this conclusion that the
class of all the bodies respecting which our simple statement
of fact (B, p 574) holds true, includes some, but not all, of
the class "living beings" But will it include only living
beings? To which the unanimous voice of the past and the
prevailing sense of the present would answer in the affirma-
tive For this view, only living bodies could compose those
progenic lines that are conditioned in VI (2) Our postulator,
however, finds reason for taking the opposite standpoint
For him it is possible that non-living bodies might be
included in such progenic lines To this conclusion he is not
forced by the structure of his postulate It seems necessary,
therefore, that we should interrupt the progress of our

commentary long enough to give some preliminary account of the reasons that impel our postulator to his position.

The motives that induced our postulator to array his opinion against that of the authoritative biologist must have appealed to him as particularly compelling. Yet it could have had no appeal for him did the issue that divided the opinion on this subject involve a question of fact. For the postulator would not be the formalist that his function in science requires him to be did he answer questions of fact on any other evidence than such as an experimental science furnished him. And, indeed, no question of fact is here involved. We assume, only on such evidence as experimental biology furnishes, that there may have been and probably were in the course of nature, certain bodies whose classification among the living and the nonliving is our only question. In short, we are approaching a critical point in our definition of life. But this question does not arise newly in the problem of postulating a biology that should condition the meaning of life. It arises in connection with the postulates of each science as they condition the essential terms of its vocabulary. This we have seen in the case of all the postulates that have gone before.

In short, we are throughout dealing with the problem, not of defining this or that particular term, but of defining what is meant by "defining."

Consider now two different understandings of the past concerning the task undertaken by a definer who would fix the meaning of a given term. To the one understanding of this past, the meaning of a term to be defined has been fixed from that moment in some possibly prehistoric past when an invariant core of denotation has been accorded the term. It remains for a definer of any later day to discover the connotation which this denotation implies. To the other

understanding of this problem, the meaning of a term, at least of a fundamental category of science, is determined by the postulate of that science to which its term is fundamental. The postulates, as we have seen, are not unchangeable and their changes depend upon the adaptation and re-adaptation of the instrument which they provide to the accomplishment of a task which is itself not entirely invariant. Awaiting a more careful retrospective analysis of this difference and of other matters related to it, an historical example may serve to sharpen the contrast. In the first class of definers we may picture Socrates questioning in the market place the various uses of such familiar terms as "goodness," "beauty," etc., and, noticing their differences, seeking with his sureness to find what connotations remain constant throughout these differences that might be considered *the* connotations of the term in question. In the other class we think at once of the word furnished by the editors of Aristotle's logical treatises, to apply to the collection of them the word "Organon." It is for us as a tool that the postulates of a science are organized, and as a part of that tool the term defined is given its meaning. But of course there could be no tool if it had no function, no end to accomplish, and the end to which we devote the implementation of biology has been frequently restated in the present chapter: the immediate end, namely, of carrying us over the rough interval from the azoic to the zoic age of cosmic history. Now evidently pre-Darwinian biology could furnish us with no such vehicle; and when finally Darwinian principles were accepted, to most biologists the fact that biology could serve to traverse this rough road appeared as a supplement to this pre-Darwinian meaning. Perhaps we could not better sum up this brief anticipatory account of the postulator's position than to turn once more to the

contrast of Kant, between analytic and synthetic judgments
Thus we may say that for the traditional understanding of
life, the proposition "Life evolved" is a synthetic judgment
For our postulator, it has become an analytic judgment

Where, then, are we to look for those bodies which the
traditional opinions and, as we have assumed, the still
prevailing sense of biology would class among the living,
but our postulator among the lifeless things of nature?
Consider those very elementary species of organism (as we
may still call them in the purely ethnological sense), whose
habitat is some region of nature so insulated against the
changes of the surface and of the surroundings of our
earth, as to leave them in a constant condition of tempera
ture, environment, motion, and all other forms of change
Such habitats are to be found in the utmost depths of the
ocean, ebbless, tideless, invariant in temperature Under
these stable conditions, such organisms have existed for
countless ages, evolving into nothing new, and so far as we
can suppose, having evolved into nothing old Such
organisms would meet not only the requirements of our
postulates VI (1 and 2) but all those more restrictive
requirements which would accord them a nutritive and
reproductive function In what way could such organisms
serve as an implement for accomplishing the end which our
postulator has set for biology? That is, in what way could
they have eased our progress from the azoic age to the
intervening periods, to the present man inhabited earth
consistently with the laws of physics and chemistry? This
is why our postulator has restricted the class of living to
such organisms as will have met the requirements of the
next condition imposed by our postulate, namely, VI (3)
To the discussion of this postulate let our comment now
turn

ment of fact when the class of bodies to which it refers is restricted to living bodies

The terms recapitulation and phylogeny make their first appearance in the history of biology with the evolutionary theory of life, where we find them coupled in a pithy dictum of Haeckel "Ontogeny recapitulates phylogeny" The definitions that have gone before prepare the way for the following simple interpretation of recapitulation

Recapitulation In the phylogenic chain of n links the morphology of the sequence of states composing the progenesis of members of the k-th link remains that of the sequence composing the progenesis of members of the $(k + 1)$ link [1]

If with the foregoing definition of progenic morphology in mind, we let k take on all values from 1 to n, we see that the progenic morphology of the first link recapitulates that of no antedent link, but is recapitulated in that of the second link, while the progenic morphology of the n-th link recapitulates that of no subsequent link but recapitulates that of the $(n-1)$ link, all intermediate links both recapitulate that of an antecedent and are sufficiently recapitulated in that of a subsequent link So that we may say in accord with the requirements of VI (5), the progenic morphology of every link in a phylogenic chain either is recapitulated in that of a subsequent, or recapitulates that of an antecedent link Moreover we see that the progenic morphology of any given link is recapitulated in that of every subsequent link down to the ultimate n-th link of the sequence From this it follows that the relation between recapitulated and recapitulating is, like that between progenitor and progeny, a reciprocal-transitive relation One whose leisure allowed him to examine in greater detail into the implications of this conclusion would be increasingly

sensible of both the aptness and the importance of Haeckel's epigrammatic dictum ("Ontogeny recapitulates phylogeny") In fact, the empirical determination that there are organisms whose properties conform to the requirements of VI (1, 2, and 3) is the *conditio sine qua non* of an evolutionary theory of life

But our own attention must turn to another matter, the formulation, namely, of the last definition that is required to classify various types of morphology

Calling the conditions imposed by VI (3) on a phylogenic sequence the phylogenic law, we may define as follows *Phylogenic morphology* to a finite producer-product sequence of progenic lines such that the progenic morphology specific to each conforms with the phylogenic law common to them all is to be attributed both collectively and distributively a phylogenic morphology

With this last contribution, our efforts defined four species of morphology physical, ontogenic, progenic, phylogenic

With these four morphologies at our disposal, we can put our own original question in a much more economical way We ask whether the possession of ontogenic, progenic and phylogenic morphology is a sufficient condition to include the body whose properties they are, in the class of living bodies As to the necessity of this simple condition, one sees that unless our postulator proposed to exclude hybrids from the class of the living, the lack of progenic and phylogenic morphology would not exclude membership in the life class But is not the possession of these three morphologies sufficient to constitute the body to which the properties are attributed the property of Life? If it were, the corpse of a newly expired organism would be no less living than it had

been a moment before For the operation of death, what-
ever else it may have done, will not so immediately have
destroyed the structural properties of the morphology of the
living To what term shall we turn for our definition of the
difference between the just expired organism and its corpse?
Of course, it is part of our language and has entered into our
literature, to contrast the extremeness of death with the
quickness of life, but the contrasts between rest and motion
would not distinguish the kind of rest that the dead enjoy
and the kind of motion the living exhibit from any other
kind of rest and motion When the reflective ages have
sought for those properties which serve to distinguish the
structures that may be shared by the living and by the dead,
thought has turned to functional properties In history from
Aristotle on, such functions as have suggested themselves
have been confined to the nutritive and the reproductive
As we have seen, these two functions alone are not sufficient
to complete the distinction between the living and the dead,
but may it not be that a post Darwinian biologist is able to
add a number of functional properties which might be called
upon to effect the differentiation required? At any rate, it is
natural that the thought of our postulator in seeking for the
properties which, added to the structural, are necessary to
distinguish the living and the dead, should turn to the
functional This thought will as usual be introduced with
what purports to be a simple statement of fact, namely
(D) There are to be found in Nature bodies meeting the
conditions VI (1–4), among which bodies some will be
living, having the three morphological attributes, onto-
genic, progenic, and phylogenic morphology, and in addi-
tion having the three functional attributes, ontogenic,
progenic, phylogenic function
Comment upon this statement of fact may safely sacrifice

accuracy of detail to brevity of result. For in the greater part of the argument, our thought will be retraveling old roads. A starting point of this reminiscent journey will be the conditions imposed in Postulates III and IV upon the categories of potential and functional production. The conditions imposed by Postulate III may be most clearly and briefly expressed in this form. Any member of the morphological X-class that is neither as yet a producer nor a nonproducer has the property of potential production, if and only if there are three members of the morphological X-class, two of which are actual producers of a y, and the other an actual nonproducer of a y. Now from the sequence of bodily states composing the last body included in a phylogenic sequence, let us select three discrete states, including the first but excluding the last. Next from the sequence of bodies composing the last link of a phylogenic chain, let us select three bodies again including the first and excluding the last. Finally from the sequence of links composing a chain let us select three links, again including the first, and excluding the last. To pass from this collection of selected conditions defining structures classified morphologically under producer-product relations to functional properties is a problem whose first steps have been covered in Chapter 19. To ease all steps of the process involved in constructing the functional properties definable in post-Darwinian terms, it will be convenient to introduce *ad hoc* terms which are most easily defined in algebraic fashion. Two situations arise in the course of our further reasoning which may be respectively represented by the formula "x_1 produces y_1 produces x_2;" second, "x_1 produces y_1 produces z_1." In the first situation, y_1 produces a structure of the same morphology as that which produces it. It seems appropriate therefore that we should speak of y_1 as having transmitted a

structural property from its producer to its product and therefore we may call it a "transmitter" In the second situation, y_1 produces a product of morphology different from that which has produced it and in this case, may we not suitably call y a "transmutor" of the morphology of its producer into that of its product? With these two terms added to our vocabulary, it is not difficult to grasp the following properties of states, bodies, and links

(a) all the bodily states of a single organism, however they may differ in their physical morphology, retain the same ontogenic morphology Therefore, the second and the third of the three discrete bodily states which enter into our first group of conditions are actual transmitters, while the first of these three, being the product of a bodily state belonging to its immediate progenitor, is not of the same ontogenic morphology as the first of the bodies which it produces For though here the producing and the producer are of the same progenic morphology, they are not of the same ontogenic Therefore the first of our three selected conditions is an actual nontransmitter We have here then three structures of identical (ontogenic) morphology, of which two are actual transmitters and the third an actual nontransmitter, and therefore the fourth which is neither to be classed with the actual transmitter nor actual non-transmitter, will be a *potential* transmitter, that is to say, will have the potentiality of belonging to the ontogenic sequence of which it is for the moment the final term

(b) our selection of three bodies composing the same link presents us with a case in which the ontogenic morphologies of these bodies may vary but the progenic morphology is constant Therefore the second and third of these bodies will be actual transmitters of the progenic morphology common to all bodies composing the link, while the first

term of this link, being the product of the body of its ante-
cedent link and therefore possessing a different progenic
morphology from that of the term we are considering, will
be an actual non-transmitter And so, as before, the fourth
term of our selection, that is, the final body of the link we
are considering, will have been the product of bodies of
which one is an actual non-transmitter, the other two actual
transmitters of a progenic morphology Thus two of the
producers will have been actual transmitters, the third an
actual non-transmitter This qualifies the last term of the
progenic sequence to be a *potential* producer of bodies of like
progenic morphology as those that constitute the link, in
other words the last term has the potentiality of producing
progenitors, and so prolonging the link

(c) Finally we have the bodies of three links composing
the phylogenic chain, of which all links share the same
phylogenic morphology but differ in their progenic mor-
phologies Further, each link of the chain except the first
and the last, will have been an actual transmutor of
the progenic morphology of its own producer, and of its
antecedent link, into the progenic morphology of its sub-
sequent link Of our three selected links, therefore, two will
have been actual transmutors, but the first, having been
produced by no antecedent link and therefore provided with
nothing to transmute, will be an actual non transmutor As
for our fourth link, which is the last of those composing the
phylogenic sequence we are considering, it will not as yet
have been an actual transmutor nor an actual non-trans-
mutor It will, according to the general formula, have
acquired the properties of being a potential transmutor,
that is to say, of continuing the evolutionary process which
has characterized the links of the phylogenic chain from
beginning to end

A word as to that decision of detail which we have sacrificed The preceding arguments assumed that under conditions specified we may attribute potential phylogenesis to the last link of the chain composing it, potential progenesis to the last body of the link to which it belongs, potential ontogenesis to the last state of the last body of the last link of that chain

But conclusion (d) of Postulate VI (5) attributes under the same conditions all three of these potentials to the last bodily state of the last body of those composing the last link of the chain A strict logician could have conducted our arguments in terms of bodily states alone, but one sees how really tedious the process would have been Whereas we have taken the shortcuts of assuming the obvious namely, that of all structures that enter into the composition of a phylogenic chain, the only one that has as yet produced nothing, and therefore cannot be classified as an actual producer of anything, is the last state of the last body This alone of the whole sequence of states faces the future with no production of any kind yet made actual, and therefore whatever potential of production that we attribute to the links or chains must be concentrated in this one Postulate VI (5) requires that the structure to which we have attributed potential productivity have the function of production This condition is that the chain c which we have been considering, should coexist with a chain c′ of a contradictory phylogenic morphology, i e , should have morphology incompatible with that of c This condition could be fulfilled only if the ultimate values of c and c′ respectively were of incompatible ontogenic morphology Then we should have two bodies of different and incompatible morphology, both of which had the same potentiality of production, and with this conclusion we may indeed say that the final state of the

final body of the final link of two phylogenic chains must possess the three *functional* properties of ontogenesis, progenesis, and phylogenesis. We may therefore, in the future, for brevity of expression, characterize the bodies as "strictly functioning bodies."

We interrupt the postulator at the end of this penultimate condition to the full conditioning of the Life concept, to ask two questions. Does he regard only living bodies to belong to the class of strictly functioning bodies? To this question he is prepared to answer in the affirmative. But what of the converse? Do only bodies possessing the triple functions of ontogenesis, progenesis, phylogenesis belong to the class of the Living? If so, he must be prepared to exclude hybrids from that class, for hybrids have only the function of ontogenesis, being without progeny, and therefore having a place in no link of a phylogenic chain. It would however require an incredible temerity on the part of our postulator were he to accept the horse and the ass as living bodies but the mule as lifeless. Nevertheless, has he not the same motive for doing just that as inspired him in an earlier comment to exclude from the class of the living the purely progenic sequence, seeing that this too fails to find a place in any phylogenic chain, and therefore fails to contribute to the course of organic evolution? But before we come to the question, "why should he?" we must ask, how could he add so to his postulates as to include the noncontributing hybrids with the contributing, strictly functioning bodies that belong in a phylogenic sequence? It is in answer to that question that he proposes to add a fifth condition to the four that have already been discussed. As usual, the topic may be introduced in the form of what purports to be a simple statement of fact:

(E) There are to be found in nature producer-product

sequences of bodies of which each subsequent body is formed of its immediate antecedent and each sequence contains, among its members, bodies that compose a phylogenic chain

To a sequence meeting these conditions our postulator has given the name biogenic line and proposes that all bodies composing and only such bodies as compose a biogenic line are to be included in the class of living bodies

In imposing the conditions where each subsequent body of a biogenic line should be born of its immediate antecedent, our postulator is not using the words "to bear" in the strict biological sense which would imply that production by birth can only describe that kind of production in which both producer and product were living beings Had our postulator so intended the term, he would have fallen into a patent circular definition Instead he has attempted an economy in what would otherwise have been a very entangled wording by using the term "birth" in a limited range of its wide application in general conversation namely, the case in which one would speak of the cells of a unicellular organism produced by fission, and the offspring drops of a parent rain drop also produced by fission as equally a sample of production by birth In both cases each offspring body begins his individual history at a moment in which it has just been severed from the parent body, what our postulator's meaning of production by birth excludes, is any case in which the product has never been a part of the body of the producer

In his comment on VI (1), the postulator has explained his reasons for abandoning all attempts to distinguish between the living and the lifeless in terms of a difference between any form of ontogenic law and any form of norm that might be attributed a mechanism, or other inanimate

body With the same comment, the postulator ventures the opinion that in the end it would be found that the differentia between living and dead consist not in the manner of respective individual lives but in their heredities Among the bodies composing a biogenic line and only in such bodies do we find those that would compose a phylogenic chain The living are to be distinguished from the lifeless forms by the conditions imposed by VI (4)

To come back to our test case how, namely, would our postulator include a hybrid in a biogenic line and therefore in the class of living beings? The answer is now plain both of the mule and the ass Prior to the mating of the horse and mare, the last terms in the two phylogenic sequences have the three potentialities of producing either a prolonged ontogenesis, or a sequence of progeny, or the progenia of a new progenic sequence When however, the two are mated, the horse begets and the mare gives birth to an offspring which continues neither of two phylogenic lines of the parental lines which cross at this point Instead there begins a new course of ontogenesis of which the initial stage is invariant in morphology with that with that which is descended from the first phylogenesis of the sequence On this protogenesis we may suppose follows a metagenesis that conforms to a norm inherited from its various ancestors which would be analogous to the vastly more complicated norm defined by the Mendelian laws that controlled the metagenesis of both its parents In that this offspring conformed to a law in its ontogenic course, it would be distinguished in no wise from instruments of the arts and even natural phenomena such as the movement of the clouds It is in recognition that such laws as the ontogenesis of the hybrid follows are inherited, that it owes its claims to be counted among the living To the conclusion of this comment we may consider

ourselves to have gathered all that is to be learned from the
intellectual experiments we have posed

The problem subjected to such experimental issue is this,
do we find in nature bodies that conform to the require-
ments of the postulate proposed in VI? The answer has been
affirmative

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[1]The biologist will recognize in our neologism "protogenic" a re
placement made solely for our present convenience of the term which
he himself uses to stand for this initial period of organic development,
namely the period of differentiation

[2]Editorial note At this point the transcription from the tape became
quite difficult The sense in which the author uses the term "recapitu
late" is made clearer in the next paragraph

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